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CLINICAL LECTURES

ON

SUBJECTS CONNECTED WITH

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AND

OBSTETRICS.

BY VARIOUS

GERMAN AUTHORS.

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PROGRESSIVE BULBAR PARALYSIS

AND ITS RELATIONSHIP TO

PROGRESSIVE MUSCULAR ATROPHY.

BY

PROF. A. KUSSMAUL,
OF FREIBURG, IN BRESLAU.

THE patient whom I introduce to you to-day, is, as you perceive, unable to make himself understood by word of mouth, notwithstanding all the effort he puts forth. It is not that voice is wanting to him, or that he suffers from aphasia, as in cases of recent apoplexy; he still finds within himself the words ready for his thoughts, but he can no longer give utterance to them, because the muscles of articulation in the tongue and lips refuse him this office. His power of articulation is not quite destroyed, he distinctly gives utterance, slowly and with great effort, to the majority of separate vocal sounds, and even to some few small words, *e.g.* "ja," and "nein," but this is all. This is the limit of his conversational powers; any attempt to answer a question is followed by excitement; he first tries in vain to squeeze out words, and then hastens to fetch paper and pencil, in order to make himself understood in writing, which he does intelligently. You see, Gentlemen, before you an excellent example of so-called "alalie," or "anarthrie" (Leyden).

It can easily be shown that a great part of this alalie has its foundation in a very severe paralysis of the tongue. You notice the somewhat atrophic, flabby, tongue lying flat on the floor of the mouth, its dorsum covered with shallow depressions. If you ask the patient to execute movements with it in various directions, these, for the most part, are either not made at all, or only incompletely. The lateral movements are completely abolished, the patient can no longer make a channel of the dorsum of his tongue, the movements upwards towards the hard palate are very limited; he can slowly protrude the tip somewhat beyond the margin of the lips, this action causing the fleshy substance of the tongue to fall into fibrillary twitchiness; he can move the organ backwards and downwards only with great exertion. The faradic excitability of the tongue is preserved, you see how promptly it curves towards the sides to which I apply weak currents.

In addition to this lingual paralysis with atrophy, a less pronounced paralysis of the sphincter muscle of the mouth, without atrophy, may be shown to exist. Our patient can, it is true, still point the mouth, but can neither whistle nor blow out a light. The lower lip does not at present hang down, as is the case in perfect paralysis; the patient usually keeps his mouth closed. The faradic excitability of the orbicularis oris is well preserved; the reaction to galvanism, indeed, is increased, four Siemens' elements cause convulsive movements on closing the circuit, not less is the reaction to mechanical stimuli augmented, every light tap which I make on the lips with a fine rod is followed by a convulsion. In the other muscles of the face I can find no paralysis.

The soft palate and the uvula have their normal position, and perform the usual movements, both in sounding the A* or Æ, and in making efforts to swallow; only a slight nasal twang in the voice indicates a small amount of paresis of these parts.

It will interest you to follow somewhat more in detail the derangements of articulation. Of the vowels, the I only cannot be pronounced, it sounds like E; all the others can be managed. If with a rod you press down your tongue on the floor of the mouth, you perchance place yourself in this man's condition; you can no longer succeed with the letter I, for the pronunciation of which the tongue must raise itself to its highest against the hard palate. Of the consonants there are wanting a good many in the alphabet of this paralytic. The R is wanting, for he is no longer able to throw the tongue into the necessary trembling vibrations. There are wanting the three so-called dumb explosives, P, T, K, and of the loud ones G, whereas he pronounces B and D. For the formation of P and T, which demand a more energetic tension of the lips than the B and D, the orbicularis is already too weak. For the formation of G and K the tongue must be pressed back to the palate, therefore they can be as little successfully pronounced as the letter I. He can still give utterance to the analogously sounding aspirates Ch and J; for in this case a way remains open between the tongue and palate, and they do not require the tongue to be so forcibly raised as G and K. Whilst the aspirates W and H succeed, the aspirate F fails, for the formation of which a close pressure of the lips to the teeth is necessary; and this the orbicularis is no longer able to perform. His F always sounds like W. The tongue can no longer manage the L, and the sibilant sounds S and Sch. The resonants M and N are both still pronounced.

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* This and the other vowels are supposed to be sounded as in Germany. —(TRANSL.)

In addition to the slight nasal twang, there is yet a peculiarity in the articulation which ought to strike you. An indistinct aspiratory sound, like h or ch, is mixed up with all articulation. The reason thereof is clearly this, that there is left remaining between tongue and palate a wide canal, so that a larger stream of air passes through and produces this noise.

As a matter of course, so considerable a paralysis of the tongue must interfere with the power of swallowing, so far as this depends upon the tongue; even mastication must proceed with more difficulty, as the tongue must follow this act with corresponding movements. Our patient is conscious, indeed, of impediments both in masticating and in swallowing, and complains that the act of eating makes him very tired. When he eats, which he generally does with eager haste, the victuals in part fall out of his mouth, and there is then the unappetising spectacle of witnessing him push them in again with his fingers; it costs him, moreover, an obvious effort to shape the morsel with his tongue and to push it backwards. In regard to the swallowing, however, of solid food, the symptoms exhibited can only be accounted for completely by supposing also that there is, in addition, a paralysis of the pharyngeal muscles.

If the patient has, by what would seem to be good luck, brought a morsel within the range of the automatic swallowing movements, he constantly assists the action with several gulping motions. Residuary portions of the morsel remain in his mouth and pharynx. If the throat be examined with the mirror, we find numerous pieces of food, horse-shoe shaped, between the tongue and the epiglottis, and if we now cause slight gulping movements by tickling the palate, the remains of the food constantly return over the dorsum of the tongue, on both sides of the epiglottis, out of the pyriform sinuses (Schlundtaschen). The stylo-pharyngeal muscles, whose contraction in the normal action of swallowing causes these pouches to empty themselves and close up, are obviously paralysed. An uncomfortable feeling of pressure in the neck, and constriction, of which the patient complains after eating, is probably to be accounted for by the fact of portions of food remaining behind in this manner.

Liquids cause still more difficulty than solid food. Small quantities only, when slowly and cautiously swallowed, pass down safely; larger ones run partially into the larynx, and excite coughing. The larynx is therefore locked only against solids, not against fluids; but the pharynx, from the nose, against both. How shall we explain the difference in the behaviour of the larynx towards solids and liquids?

The closing of the larynx in swallowing is of such importance that Nature has done her utmost, and for this object has provided a double mechanism, so that nothing can possibly force its way through from the aperture of entrance.

The one mechanical arrangement, long known, which we can designate as the lingual epiglottic closure, is brought about by the interchanging displacement of the larynx and the root of the tongue, the former moving under the tongue, the latter over the depressed epiglottis. In our patient this mechanism, though still preserved, is already impaired, for, by feeling the parts on the floor of the mouth, we can convince ourselves that the movements of the hyoid bone and the larynx, in swallowing, occur with little energy. If, in spite of imperfect action of the root of the tongue, the solid morsels of food glide safely over the epiglottis, this is to be accounted for by the fact that the morsel, if it moves down from the tongue on to the epiglottis, partly by its own weight, partly by pressure to which it is subjected by the upper constrictor of the pharynx, presses the epiglottis closely down, and thus cuts off the larynx.

That, however, which is sufficient when the food is of a cumbrous, coherent nature, suffices not for fluids, the particles of which become so easily dispersed. For these the parts must remain closed when the draught arrives in front of the epiglottis; only small quantities slowly swallowed pass in front of the epiglottis and at the sides over the lower borders of the plicæ ary-epiglotticæ down into the channel of the pharynx and the œsophagus; larger quantities flow over the barriers of the larynx, unless the second, or pure laryngeal, mechanism of closure, which is altogether independent of the tongue and epiglottis, is in proper order. The existence of such a pure laryngeal closure has in former times been much disputed in various quarters, but the laryngoscope has finally settled the question in the affirmative. In the case of a girl whose epiglottis was completely destroyed, but who could eat and drink without any of the food going the wrong way, von Bruns could distinctly see that, at each attempt at swallowing, the entrance to the larynx was firmly closed by the tight shutting together of the arytænoid cartilages, and of the ary-epiglottic folds proceeding therefrom. On the other hand, in the case of a man with strongly marked dysphagia following general muscular atrophy, which had also attacked the œsophagus and the muscles of the larynx, he noticed that small quantities of milk, when gulped down, at first remained in the pharyngeal pouches, and when with a further supply of the liquid its level was raised, that the milk flowed through the rima glottidis posterior into the cavity of the larynx, and immediately brought on an attack of coughing. Besides the transverse arytænoid muscle, which brings together the cartilages of that name, there are, in particular, fasciculi of the thyreo-ary-epiglottic and thyreo-arytænoid externi muscles, which bring about lateral compression of the passage into the larynx to such an extent as entirely to close it. A paresis, therefore, of these laryngeal muscles is the cause why, in our patient, water when swallowed penetrates so easily into the larynx.

Not only those laryngeal muscles which are subservient to the function of deglutition, but also those connected with phonation, are already somewhat paretic. The mirror shows us that the movements of the vocal cords when a note is sounded are the result only of great effort, and not of gentle attempts at intonation. I shall, further on, return to the consideration of how it is that the power of voluntarily coughing and hawking has been lost to the patient.

In conclusion, there is one very troublesome symptom, viz. the increased accumulation of saliva and mucus in the mouth and throat; when he speaks, but even without such a cause, the saliva from time to time shoots forth from his mouth. The ejection of saliva appears in our case to be simply the result of the dysphagia; an experiment, by which the daily quantity of saliva discharged was accurately measured, indicated no increase of secretion.

You will, Gentlemen, have already discovered that the paralysis of which our patient is the subject, has, as is very remarkable, attacked to a notable degree no other muscles of the face and neck but those which are concerned in articulation and deglutition, also, in a minor degree, those concerned in phonation. Those muscles belong to certain sets of nerves—the facial, the hypoglossal, the spinal accessory, perhaps also the glosso-pharyngeus. Of the numerous muscles supplied by the facial, the paralysis has sought out only those which are subservient to articulation, viz. the orbicularis oris and the levator palati, this last, up to the present, only to a slight degree. On the other hand, it has spared all those muscles which serve only for purposes of gesture, for closing the eyelids, and for mastication. It is, moreover, very remarkable that no sensory nerve has suffered, smell, hearing, taste, and the tactile sensibility of the face and in the mouth and nasal cavities, are quite unimpaired. We have therefore before us a pure motor paralysis confined to muscles of a like function. Obviously this paralysis affecting speech and swallowing, which in a very symmetrical manner pervades the muscular structure of both halves of the tongue, lips, and larynx, is not dependent upon the topographical arrangement either of the muscles of the face and neck, or that of the nerves at the base of the skull, but deals with the functional value of muscle and nerve. We are thereby distinctly referred to a central source of the affection, and the conservation of the faradic excitability in the paralysed muscles supports this assumption. There must be perfectly defined motor centres serving for articulation and deglutition, whose function has been paralysed by some morbid process or other. We look for these centres, as you are aware, in the bulbus medullæ. Here, on both sides of the rhombic fossa (in the floor of the fourth ventricle), lie, as Stilling has discovered, the gray nuclei of the hindmost motor nerves of the brain, most posteriorly those of the hypoglossi, further forward that of the spinal accessory and facial, exactly, therefore, those of the nerves which are here concerned, while the nuclei of the sensory nerves, here unaffected, which have their roots in the bulb of the medulla, are found on the outer side of it. If the nuclei of the hypoglossi, and the ranges of the accessory and facial nuclei, which are closely connected therewith functionally, and therefore also anatomically, are destroyed, or the fibres connecting these nuclei with the brain interrupted, the result may be such circumscribed paralyses of those muscles and nerves which set in motion the machinery of articulation and deglutition. We have before us a glosso-labio-laryngeal paralysis in the sense of the French physicians, a bulbar paralysis according to the signification preferred by the majority of the German physicians, a paralysis of the bulbar nuclei, if the paralysis proceeds from the above-named motor bulbar nuclei.

In addition to what has been described, we find in our patient only one muscular region in a state of progressive muscular atrophy. You observe that the ball of the thumb, and a few interossei of the right hand, are slightly atrophied. The patient is conscious of a weakness of the hand, but can, however, still sew with it. The faradic excitability is preserved. In other respects no bodily function has suffered; appetite, digestion, defæcation, evacuation of urine, sleep, respiration, and circulation, are none of them disturbed.

It now remains to us to make ourselves acquainted with the development of our patient's disease. In the first place a definite assertion can be made with reference to the nature of the process which lies at the basis of the paralysis. Has the affection had a sudden commencement and been of an apoplectic kind, or have there been acute and febrile symptoms? and has the paralysis rapidly reached its present height, or has it gradually commenced either without or with slight symptoms of excitement, and undergone a progressive development up to its present state, possibly also with fluctuations? We must, in the first place, answer these questions.

Our patient, a tailor from the country, 32 years of age, never previously ill, though of weak bodily conformation, distinctly traces his ailment to a cold caught whilst at work in the fields in September, 1871. He was attacked with headache and pain in the neck, and eight days later he was conscious of difficulty in swallowing and speaking; from the commencement of the dysphagia his mouth was full of water. The headache disappeared within the first eight to fourteen days; the pains in the neck lasted much longer, and returned from time to time at certain intervals during the first months. By the new year his speech had already become almost unintelligible. In April, 1872, the patient was for the first time conscious of a certain amount of weakness in the right arm. There had never been any fever. The appetite of the patient was always good, and this increased until he was tormented by hunger, when the dysphagia became more marked, and the patient emaciated, in consequence of not being able to take a sufficiency of nourishment.

On the 23rd April, 1872, the patient for the first time sought relief in our clinical wards. At that time he was able to pronounce all sounds with the exception of the R and Sch; those, therefore, which, as being the most difficult, children are accustomed to learn last, were the first to be lost by him. Of the other sounds he experienced the greatest difficulty with T, F, P, S, and L. He still succeeded in pronouncing slowly separate words and little sentences, and one could still always pretty well guess what it was that he wished to say. The tongue was already slightly atrophied, and its movements towards the sides and upwards towards the palate very considerably checked; there was less hinderance to its forward movement. Treatment by galvanism was followed by a moderate amount of improvement in the speech, a still greater amount in the power of swallowing, and there was greater freedom in the movements of the tongue. In the summer the patient stayed with relations in the Black Forest, where good living agreed with him. In autumn he returned to poor diet at his own home; his condition soon altered for the worse, and he therefore, on the 27th November, 1872, sought admission into our Hospital.

We have, therefore, Gentlemen, to deal with a primary and independent paralysis of articulation and deglutition; this is not connected, as is more often the case, with an already existing cerebro-spinal or spinal affection, a multiple scattered sclerosis or progressive muscular atrophy, general paralysis, tabes, and the like, and the slight muscular atrophy on the right hand, which reminds one of progressive muscular atrophy, developed itself half a year later than the paralysis of the tongue.

It is also not an instance of an apoplectic affection or of a paralysis of speech and swallowing of an acute form, such as may be the result of hæmorrhage, embolic, thrombolic, and inflammatory softening of the brain, the pons, or medulla oblongata, if the conducting fibres between the organ of the will and the bulbar centres for those functions are interrupted in their continuity. In such cases it can scarcely be a simple paralysis limited to deglutition and articulatory speech; it is always evident that other channels for conduction are broken through; according to the situation of the diseased area we have the existence of hemiplegia, paraplegia, lagophthalmos, etc.; the lingual-labial-laryngeal paralysis attains its maturity suddenly or rapidly; no atrophy of the tongue then occurs, and the capacity of the paralysed parts for reflex movements remains, notwithstanding the paralysis of voluntary movement.

Our patient suffers from a *chronic* affection; quite gradual, with slight initial symptoms of excitement, viz. headache and pain in the neck; it has originated and gradually gone on developing, the pain in the neck recurring at shorter intervals. The type you see before you is one of advancing paralysis of the tongue and lips, the "paralysie glosso-labio-laryngée progressive," or progressive bulbar paralysis. I think it may be more precisely designated as progressive paralysis of the bulbar nuclei, as a paralysis dependent upon a slowly advancing degenerative process, which destroys, by atrophy, ganglion-cell after ganglion-cell in the hypoglossal nuclei, and the masses of ganglion-cells of the spinal accessory and facial nuclei functionally connected therewith. The initial pains of the head and neck, so far as they indicate an irritative hyperæmia and tumefaction of the affected bulbar regions, make it appear probable that this process is of a myelitic kind, just as in the majority of the cases of a similar kind, which, up to the present time, have been fully investigated, the products of a chronic degenerative myelitis were discovered.

When the progressive bulbar paralysis develops itself so clearly and gradually as in our patient, we can scarcely go wrong in our diagnosis. At first, however, a tumour compressing the posterior part of the rhombic fossa might induce a similar type of paralysis of the tongue; but this would scarcely terminate here without far more violent symptoms of excitement (neuralgia of the fifth nerve, spasm in the range of distribution of the facial, hypoglossal, accessory, and so on); and just as little could it end without exhibiting the effects of pressure upon the adjoining regions of the medulla oblongata and the roots of its nerves (painful anæsthesia, ageustia, deafness, paraplegia, hemiplegia, lagophthalmos, and so on); if the pressure affected the extra-medullary nerve-roots, for example one or both hypoglossi, the faradic excitability of the muscles concerned—that is, one or both halves of the tongue—would soon become deficient, whereas the tongue of our patient was capable of prompt reaction. By the assumption of a tumour we could scarcely explain the later occurrence of shrivelling of the muscles of the right hand included in the gradual advance. This, on the other hand, could be easily conceived possible, if the degenerative process begins to develop itself in the cervical spinal marrow in the same way as it has done in the rhombic fossa.

So long as in our patient the destructive process was mainly confined to the limits of the hypoglossal nuclei, and the adjoining nuclei of the accessory were but slightly involved in the degeneration, the only danger which threatened the patient was that of exhaustion from insufficient supply of nourishment, which could be prevented for some time by artificially feeding him with the aid of an œsophagus tube. The degeneration, however, of the nuclei of the accessory is certain to lead to a fatal conclusion, either by asphyxia, or by attacks of syncope.

The form of the disease of progressive bulbar paralysis which you have just seen, Gentlemen, is one of those which can never be forgotten when once brought before our eyes. Whereever at any future time you may again meet with it, you will recognise it forthwith; for there is no disease of which the characteristics are more marked. So much the more remarkable is the fact, and it is one which certainly does not testify to a high degree of perfection of pathological discipline, that Duchenne first made it known to science, and in the year 1860 sketched the first illustration of it. The cause of this has not been that the scarcity of the affection has been too great. We are already now in possession of the accounts of more than fifty cases, and I myself, in eight years, have been consulted five times by persons suffering from this affection; of these five, three came from Freiburg and the country in the immediate neighbourhood.

That the affection was noticed before Duchenne is quite certain. Trousseau, as early as 1841, noted down in his day-book a case which belongs to our subject, but it remained, as he himself expresses it, only as a dead letter for him. So it was with others, they could neither interpret their observations nor turn them to account for purposes of science. Many saw therein only a variety of progressive muscular atrophy; as for example, Duménil of Rouen, who, in 1859, reported a case where he had made a post-mortem examination, in which such a progressive bulbar paralysis, and that without atrophy of the tongue, ran its course along with progressive general muscular atrophy. Others, again, attached all importance to the bilateral hypoglossal or facial causation of the malady, the lingual or facial diplegia, and while regarding the separate symptoms they overlooked the general disease. In conclusion, a great number contented themselves with vague diagnoses, such as apoplexy of the tongue, paralysis of the tongue, softening of the brain, and the like. At the present day, the knot of symptoms of the progressive paralysis of articulation and deglutition, as it has been portrayed, in its principal features, by Duchenne, is universally recognised as a typical form of disease, as a peculiar kind of paralysis. Following Trousseau's example, the French have hitherto usually designated it as "Paralysie labio-glosso-laryngée," or, more briefly, as "Paralysie glosso-laryngée progressive"; we in Germany have, as proposed by Wachsmuth, given to it the more appropriate name, and one which, at the same time, points to the site of the disease, of progressive bulbar paralysis; it would be still more correct to call it progressive paralysis of the bulbar nuclei.

We must now in the next place pay attention to what may be asserted with more precision concerning the origin, symptoms, and cause of this remarkable affection, founding our observations on numerous cases which have been recorded since 1860.

The persons who are attacked by progressive bulbar paralysis in its typical primary form, and not merely in the later stage of a progressive muscular atrophy, or of a disseminated sclerosis of the brain and spinal marrow, and so forth, have, with rare exceptions, exceeded thirty years of age, and the tendency to this disease appears to increase as age goes on. If I omit two cases, as to which it is doubtful whether they belong to this subject, inasmuch as they deviate in essential particulars from the ordinary form of our ailment, one case by Wachsmuth, which occurred in a girl aged 17 years, and another by Frerichs, in a boy of 10, there remain of forty-three patients whose ages have been given, only one person from 20 to 29 years of age, six from 30 to 39, eight from 40 to 49, eleven from 50 to 59, fourteen from 60 to 69, and three from 70 to 72. Of fifty-three persons whose sex was mentioned, there were thirty-four men and nineteen women. The disease has been observed in the lowest and highest ranks. It seems, indeed, to occur rather more frequently among the better situated classes. Hereditary transmissibility has not at present been proved.

The specific origin of the disease is, in the majority of the cases, obscure. As a rule the persons have been previously healthy and strong. Catching a cold and psychical changes have been very commonly charged (as in one of my cases) with having caused the disease; there had been in one case a fall upon the head; in another, excessive tobacco smoking; in a third inveterate rheumatism. The disease was repeatedly found associated with syphilis. In one remarkable case of Stein's, it appeared that the disease had been occasioned by violent muscular exertion, as is the case also with progressive muscular atrophy. A weaver, 53 years of age, played the clarionet and bombardon on the same night in two villages far apart from each other, and made in the interval a march of many hours in severe cold. On the following days articulation and deglutition were affected.

The symptoms with which the disease commences are, for the most part, so trivial, that the patients do not seek the physician's advice; they have no suspicion of the terrible lot which awaits them.

As a general rule, there are no precursory symptoms to announce the disease; it steals upon its victim very imperceptibly, and reveals itself primarily, and only by slight troubles in articulation, which are subsequently, and generally indeed speedily, accompanied by difficulty of swallowing; but these latter symptoms often occur simultaneously with the former ones. At the same time the patients feel themselves quite well, they only find the tongue heavier, and that more force is required to be used to set it in motion in speaking or eating; both of these actions cause great fatigue. Certain vocal sounds, particularly the R, are peculiarly difficult. Persons around them soon notice that pronunciation is altered, and that it is no longer so distinct as before. As a rule, the tongue is first attacked, later come, in turn, the lips, the larynx, the pharynx, and the palate. Exceptions to this course of the paralysis are rare, yet among his first cases Duchenne described one in which the paralysis commenced in the palate and lips. According to Duchenne, the commencing paresis of the lips sometimes reveals itself as a sensation of slight stiffness, such as might be caused by frost. In one case of Hammond's, where the paralysis is said to have commenced at the lips, the first symptom noticed was that the cleft of the mouth was more than usually prone to remain open. With the difficulty of swallowing, a troublesome ejection of saliva is apt soon to occur; in exceptional cases (Scholz, Fournier, Thilesen, Huss) a copious secretion of saliva precedes the paralysis of deglutition. It cannot, therefore, in such cases be a simple question of a retention of mucus and saliva in the mouth as a consequence of dysphagia. Such slight symptoms of paralysis remain in rare cases stationary for months, and even years, until they suddenly become rapidly serious in consequence of a violent cold (Leyden), or without any demonstrable cause (Charcot).

Only as exceptions do these initial symptoms come under observation in convalescence from febrile disorders; they have appeared after a febrile delirium lasting three days (Trousseau), and after a pulmonary fever with violent headache (Webber).

It has been asserted that the disease may make its appearance suddenly in the form of an apoplectic attack, and Wilks has been appealed to as an authority; but the bulbar paralyses, the occurrence of which was several times observed by Wilks in cases of apoplexy with and without hemiplegia and paraplegia, and which continued after the hemiplegia had subsided, were paralyses of a complete, and not of an advancing kind. It is also improbable that, in a patient of Duchenne's and Joffroy's, an apoplectic attack, with loss of consciousness and paralysis of one arm, remaining for several days, should have been connected with the progressive paralysis of the tongue, which it preceded by six months. On the other hand, in one of my patients, a very strong, and previously always healthy Catholic priest of 60 years of age, of regular habits, the disease commenced in a manner which may be styled apoplectiform. After having suffered for eight days from pains in the back, without any known cause for the same, he found, in March, 1868, while preaching, that his mouth was somewhat distorted, and that it had become more difficult for him to speak; nevertheless he was able to finish the sermon. There neither was, nor had been, any giddiness. From that time there remained a feeling of heaviness in the tongue. After a catarrh which lasted for several weeks, with violent cough and copious mucous expectoration, which subsided only very slowly, his condition became rapidly worse; he stammered in his speech; there was great dysphagia; both arms became weak; attacks of prostration, going on almost to fainting, occurred, particularly at night. So early as September, 1868, the patient exhibited the sad picture of confirmed bulbar paralysis, with paralysis of both arms and atrophy of the small muscles of the hand. He died on the 18th September, 1869. The post-mortem examination revealed, neither in the medulla oblongata nor in the pons Varolii, any macroscopic residue of a so-called apoplectic deposit. Products of degenerative myelitis were found in the anterior and lateral white and gray districts of the spinal cord. The medulla oblongata was not examined microscopically.

The disease had a very peculiar commencement in a female patient of Leyden's, who, with the exception of being subject to frequent attacks of vomiting and headache, had been previously healthy. In her case, without any premonitory symptoms, an attack of dyspnœa suddenly came on, and this lasted for five minutes. A few days afterwards she noticed a difficulty of moving the tongue in speaking and eating.

As initial symptoms, however, headache, pains in the neck, feelings of constriction in the throat and of tightness of the chest, have sometimes been noted; these, perhaps, may be regarded as symptoms of excitation consequent upon hyperæmia of the medulla oblongata. I am also inclined to refer the initial apoplectiform symptoms which occurred in the priest's case to a rapidly increasing hyperæmia. In a gardener's wife of this place, 47 years of age, strong, and previously always healthy, who had suffered much from troubles, the disease commenced neither with headache nor pains in the neck, but with an unpleasant sensation in the malar regions in front of the ascending ramus of the lower jaw on each side; this was soon accompanied by a feeling of weakness in speaking. If she made an effort to speak, there was a sensation of tightness in the upper thoracic regions, with constriction in the throat. Gradually the speech became indistinct, biting and swallowing became difficult, but not till a year afterwards did fluids pass into the larynx, and the patient's speech become unintelligible. At each menstruation, which was very abundant, the symptoms became worse, while in the intervals her condition improved.

As the disease becomes further developed, the order of succession in which the vocal sounds are lost to the patient's alphabet is not in all cases the same. This is determined by the order of succession in which the muscular regions of the departments of articulation—the lips, the tongue, the palate—are invaded by the paralysis. If the lip articulation is the first to be lost, O and U will be the first to be found impossible to pronounce; later on, I and E; A remains generally as long as any power of phonation exists. In cases where the tongue, before the lips become paralysed, loses the power of raising itself from the floor of the mouth, the I will be the first sound impossible to pronounce, as we are taught by the patient who has been presented to you. The loss of the tongue articulation deprives the paralysed, in the first place, of the R and Sch, then of S, L, K, G, T; later on of D and N. Paralysis of the lips first makes it difficult to form the letters P and F, later on B and M, and finally W. The paralysis of the palate is not only the cause of the nasal twang, it also prevents—when it has attained such a degree that too great a current of air escapes from the nose—the formation of the lip sounds B and P, which now sound as Me, We, or Fe, because, as Duchenne observed, the strength of the current of air which must pass through the mouth and lips is short of that which is necessary to overcome the contractile tension of the latter. If this tension be still practicable, the patients can pronounce B and P if the nose be closely shut; a fact which establishes beyond doubt the correctness of Duchenne's view. If speech is completely lost, all that at last remains is the power of uttering grunting sounds, or even this is no longer possible. Aphonia is always a concomitant of alalie in its most advanced grades.

As is the case with the interruptions to speech, so also the difficulties connected with swallowing take a different shape, according as the various muscular departments which are concerned in the act of swallowing are attacked by the paralysis in a different chronological order, and with varying intensity. When there is incomplete closure of the lips, the saliva continually trickles down over the lips and chin. In order to catch it, the patients constantly hold their handkerchiefs in front of the mouth as long as their arms possess the necessary strength. Even the falling of the food out of the mouth is a consequence partly of the insufficient closure of the lips, partly of the paralysis of the tongue. The more the tongue loses the power of seconding the masticatory movements, by moving about as required for the purpose, and of mixing up the saliva with the food, and so rendering it mobile; of curving itself in the form of a shovel in order to receive the chewed morsel; and, finally, of pushing the mouthfuls from before backwards into the throat, by pressing them against the hard palate, the more difficult does the first act of deglutition become. Even without paralysis of the buccinators, the morsels of food remain between the rows of teeth and the cheeks, if the tongue can no longer draw them out; they also remain in the front of the mouth and fall therefrom, even if the lips have still a tolerable amount of strength, as soon as the patient, in the hurry caused by his canine appetite, pushes in fresh morsels before the old ones have been got rid of. The patients endeavour to press the tongue anteriorly upwards, by using the forced aid of the mylohyoid, which, as a rule, remains unparalysed for a long time, or even to the termination of the case, and which, as you know, is supplied by the fifth pair; or they even assist themselves with their hands, push the food deep down into the mouth and over the tongue, press the floor of the mouth upwards, squeeze the lips and cheeks together, drink also more frequently, and throw the head backwards. In cases where the paralysis of the tongue is very much in advance of that of the larynx, we notice that the patients have recourse to victuals of a pappy and fluid kind, for these, naturally, can be more easily transmitted down the gullet. The circumstances are different when, as in the case which I brought before you, the special mechanism for the closure of the larynx is early affected. In such a case liquids get into the larynx sooner and more readily than solid food, and the patients are afraid of every beverage which regularly produces attacks of choking and coughing. If there is defective closure of the nares, first liquids and then morsels of food return through the nose; and if, as in a case of Trousseau's, the lower constrictors of the pharynx still retain their energy, drinkables and solid food may be ejected with convulsive force through the nose, and through the mouth at the same time, if the lips are paralysed. But few morsels of food in such a case reach the œsophagus. When the pharyngeal muscles are paralysed, at first the scraps of food remain fast in the furrows of the throat, and the patients try to help themselves by efforts of gulping; later on large morsels may remain in the throat, and may occasion risk of suffocation in the same way as scraps of food which reach the larynx. In the case of the gardener's wife, in our clinical wards, of whom mention has been made, the upper portion of the œsophagus at last became paralysed, and life was prolonged only by feeding her twice daily by means of the œsophagus tube.

The flow of saliva is oftentimes so abundant—the amount of saliva secreted in a patient of Schultz's was six times in excess of the normal quantity—that this phenomenon cannot be explained by the dysphagia alone. The increase of secretion is probably of the so-called paralytic origin, as, Cl. Bernard observed to occur in animals in whom the typanico-lingual trunk of the facial nerve had been divided some time previously.

In addition to the paralytic alalia, dysphagia, and salivation, the symptoms of a paralytic aphonia also most commonly occur at an early stage of the disease; these assist in aggravating the difficulty of speech, and in its final destruction. The patients are first of all compelled to use much more force in order to get their vocal cords into the right condition for producing sound, and to make them vibrate, an effort which causes great fatigue. Possibly with this was connected the strange explosive manner in which consonants were uttered by one of my patients, a manufacturer's foreman, 56 years of age, who was no longer able to make himself understood by word of mouth, but who could yet articulate more or less distinctly all the vocal sounds, with the exception of the R, S, and Sch. After that the derangement of speech and swallowing had lasted for two years, the lips and palate could be properly closed, the tongue, larynx, and pharynx having suffered more. When he wanted to pronounce B, D, and so forth, he sounded at first an M, which gradually became louder until it merged into the B or D, with sharp E, amounting to Mbé, Mdé, and so forth. As the aphonia went on increasing, the laryngoscope showed that the vocal cords were paralysed in various degrees. In the case of the gardener's wife previously mentioned, when the patient could still only groan loudly and shout with laughter, but no longer articulate syllables, the vocal cords in forced inspiration and expiration, performed with rapidity movements to correspond; even in an experiment, when she was asked to sound the letter A, they moved towards each other, but without continuing to do so, and without vibrating. In the case of the priest, who could likewise break out into loud shouts of laughter when he had already quite lost his speech, I saw the glottis open and form a wide ellipse when he was trying his powers of intonation.

To this paralytic aphonia, due to the weakness and paralysis of the muscles of the larynx concerned in voice, there is added also a series of paralytic derangements of expiration and expectoration which are probably of a bronchial nature. When the inspiratory movements, by means of the diaphragm and the intercostal muscles, are still vigorous, and when also the expiratory abdominal muscles are capable of powerful contractions in reflex attacks of coughing, induced by the penetration of water into the larynx, or in forced breathing, it is impossible for the patient either to clear the throat, to blow the nose, or to cough voluntarily. Our patient, the gardener's wife, was in great dread of gaping, for "she was then obliged," as she noted down in writing, "to groan and strain in order to get rid of the air and then breathe more freely." Duchenne and Joffroy found in a patient of theirs the inspiration to be normal, but the expiratory movement to be too brief, and, by perhaps one-quarter or one-half, too weak. This patient had a troublesome sensation of fulness in the chest, and a constant, intense craving for air; and this he endeavoured to get rid of, partly by making, in vain, forcible inspirations, which only increased the distress, and partly by energetic contractions of the abdominal muscles. Symptoms of this kind may with probability be referred to a paralysis of the bronchial muscles.

With reference to symptoms of paralysis of the vagus, we may, in conclusion, regard as such certain attacks of dyspnœa and syncope, which have nothing to do with coarse mechanical obstructions to breathing, as would be caused by the presence, in the air-passages, of saliva, mucus, and the like. They usually occur only in the advanced stages of the disease, seldom at an early period, are particularly likely to happen after exertion, though sometimes they occur at night when the patient is in bed, and death in such cases is frequently sudden. The attacks are found associated with a very great acceleration of the pulse, which may reach 130 and 150 in the minute, and with the sensation of a total failure of strength.

If you now take, in addition, a few of the symptoms connected with gesticulation which result from the paralysis of the lips, you have the most essential of the derangements of the bulbar function which are present in the disease under our consideration. In cases where the paralysis of the lips is sufficiently extensive, the mouth is observed to be increased in breadth by the action of the antagonistic muscles, and the naso-labial furrows to be deepened, and a whining aspect is given to the physiognomy. Later on the mouth hangs loosely down, the lower lip is everted downwards, and the saliva runs away over it without intermission. Frequently the muscles of the chin participate in the paralysis, but this is very seldom the case with the muscles of the facial group which are situated higher up. In conclusion, it sometimes happens that the motor portion of the fifth is attacked and the masticatory movements are affected; this was the case, for example, in our patient, the gardener's wife.

Even when the paralysis of speech and swallowing has lasted for a long time, the tongue and lips do not always become manifestly atrophied. They often retain their dimensions up to the termination of the case. Those cases only in which the muscles of the tongue, beginning at the inferior lingual and ending at the genioglossus, become gradually paralysed without suffering any diminution of size, are allowed by Duchenne to be genuine instances of his "Paralysie glosso-labio-laryngée," and these must be recognised as distinct from the "Atrophie glosso-labio-laryngée." I will, by-and-by, lay before you the reasons against making such a distinction. Most commonly the tongue is seen to be atrophied (it was so in all my cases), less often, as it seems to me, the lips are so affected; I found them to be attenuated only in the case of the priest. When the tongue undergoes diminution of bulk, it shrivels up, becomes flaccid, fills up the floor of the mouth to a less and less extent, and is the seat of fibrillary convulsions. Of its movements, the first to be lost are the grooving of the dorsum, the bending of the tip upwards and backwards, the lateral movements, and those when the tongue is raised against the palate; at last the power of protruding it is also lost. I and others were very much surprised at the great force with which the tongue, when all these movements were partly completely, and partly almost entirely lost, was nevertheless drawn backwards and downwards, when, perhaps, for laryngoscopic purposes, an attempt was made to grasp it and pull it forwards. This is to be accounted for by contraction of those antagonists to the genioglossus which are not supplied by the hypoglossal nerve; the stylohyoid and posterior belly of the digastric on the one hand, and the sterno- and thyrohyoid on the other. Only in exceptional cases do the soft palate and uvula exhibit any deviations from their normal situation. The excitability of the tongue and lips for induction currents falls only when the atrophy is marked. You can, in our patient, convince yourselves that even when the hypoglossus is galvanised only on one side, notwithstanding the advanced degree of paralysis of the tongue and the associated atrophy, every closure of the circuit is followed by a movement of swallowing, which is continued to the pharynx. This, as far as I know, was first noticed by Schulz.

We can easily conceive, Gentlemen, that the progressive bulbar paralysis, although it runs its course without febrile movements, and the digestion, as a rule, continues to be excellent until nearly the termination of the case, gradually conduces to great emaciation, with enormous diminution of muscular power, which compels the patients to spend the day in bed or in an armchair, without any proper paralysis of the extremities and muscles of the trunk necessarily occurring. This weakness is simply the result of the inanition; the appetite of the patients cannot be satisfied—they suffer the torments of Tantalus. But, as a rule, matters do not stop at a simple weakness, and symmetrical emaciation of all the muscles, but go on to actual paralysis, sometimes of more particular muscular regions, at another time of a general kind; sometimes with, sometimes without, atrophy of single groups of muscles, bearing witness to an encroachment upon central organs, partly affecting other motor portions of the medulla oblongata, and partly the spinal marrow. In the earliest stages, in consequence of the cervical spinal marrow becoming implicated, we often notice a gradual atrophy of the ball of the thumb and of the small muscles of the fingers, generally of one or both hands, exactly resembling that which occurs in progressive muscular atrophy. Often, also, at an early period, and, at any rate, as it would appear, later on, not only the expiration, but also the inspiration, is observed to decrease in energy, and this, as a matter of course, rapidly augments the general debility and hastens the fatal issue. As the disease still further advances it not seldom happens that the muscles of the head and neck become paralysed; the patients are no longer able to turn the head, which at last falls forwards upon the breast. The muscles also of the trunk, and the whole of those of the limbs, become at last paralysed, the lower ones sometimes before the upper ones; as in our patient, the gardener's wife, in whom at the same time contractions resembling those in talipes varus occurred in both legs. And as the bulbar paralysis often comes on with pains in the head and neck, so we sometimes notice that these paralyses of the extremities develope themselves with pains in the back and loins, and the patients become afflicted, even, indeed, periodically, with violent pains throughout the muscles of the limbs. What is remarkable is that the sphincter ani and the vesical muscles almost always remain unaffected by the paralysis. If this widespread paralysis of the spinal muscular regions accompanies bulbar paralysis, a condition arises which is one of the most dreadful possible. The head, trunk, and extremities are motionless, only the look discloses what is going on in the recesses of the mind, which is condemned to reflect in mute seclusion on the inconsolable miseries of its condition, and unresistingly to endure them.

We may well be surprised, that an affection which is so apt to depress the spirits, and, indeed, forces tears from many patients, conduces only in exceptional cases to mental derangement. Very frequently, however, delirium has been observed in the terminal stages.

The urine has often, but in vain, been examined for sugar and albumen. Only in exceptional cases, and only in a slight degree, has any reduction of sensibility in the fingers and hands been found to exist.

The course of the disease is always chronic, extending over one or more years. It is, as Duchenne at the very beginning insisted, pre-eminently progressive in the sense in which the word is used by Requin, that is, that once developed, its nature is to advance in a downward direction until it ends in death. As a rule, the fatal termination ensues in from one to three years. There are as yet no observations to assure us indubitably of any termination in permanent recovery; all that has been seen has been transient improvement, or the disease remaining at a standstill. The older cases of Coppette and Tommasi, which have been usually quoted in favour of the curability of the disease, are just as little demonstrative as those recently communicated by Benedikt, and of which not one corresponds with the typical form of progressive bulbar paralysis. Sometimes the patients are suffocated by gross mechanical impediments to breathing, particularly by particles of food which get into the air-passages, or by mucus which they are no longer able to expectorate from the bronchial tubes in a state of catarrh; sometimes they are killed by the above-mentioned attacks of apnœa and syncope.

II.

THAT in the progressive paralysis of the tongue, throat, and lips, we have not simply to deal with a muscular paralytic affection, as had been assumed by Duchenne in his first work (1860), had been indeed evident to Trousseau. He had, shortly after this, submitted several cases of this disease to post-mortem examination, whereas Duchenne had at his command only clinical illustrations, but no post-mortem reports. Although Trousseau's reports of post-mortem examinations are by no means exact, they nevertheless again and again establish the occurrence of an atrophy of the hypoglossus, and on two occasions a more indurated condition of the medulla oblongata. In addition Trousseau found, in a case in which there was also general progressive muscular atrophy, that there was atrophy of the majority of the spinal motor nerve-roots in addition to that of the nerve-roots of the bulb of the medulla. The same result had shortly before been obtained by Duménil in the exactly similar case which has been already mentioned. From these reports Trousseau concluded—1. That the progressive paralysis of the tongue is of a neuropathic and not of a myopathic character; 2. That it depended upon an atrophy of the bulbar motor nerve-roots, and the progressive muscular atrophy, upon a similar affection of the spinal nerves. The central character of the affection had not as yet been recognised by Trousseau. This recognition was made, somewhere about the same time, by three German physicians, Barwinkel in Leipsic (1860), Wachsmuth in Dorpat, and Schulz in Vienna (1864). All three arrived at the conclusion, theoretically, that Duchenne's paralysis emanated from the medulla oblongata. They connected this hypothesis with Stilling's discoveries and the anatomico-physiological considerations of Schrœder van der Kolk with reference to the structure and function of this organ. They endeavoured to find the site of the affection in the gray substance of the rhombic fossa, where the motor nerve-nuclei of the hypoglossus, accessory and facial, one after the other, lie together so close to the raphe, or in the neighbouring olivary bodies, with which they are connected by means of sets of nerve-fibres (the centres of co-ordination for articulation, deglutition, and expression, according to the theory of the Dutch anatomists). Here, then, as they thought, a process could be conceived to occur in a narrowly circumscribed space, which was able slowly to destroy the movements of swallowing and speaking, also those connected with expression, as in Wachsmuth's case, and which, by encroaching upon the origin of the vagus, might cause sudden death. By reason of the topographical arrangement of those nuclei in close proximity to the raphe, it could scarcely occur otherwise than that both halves should be diseased simultaneously, and that symptoms of the paralysis should appear on both sides. Wachsmuth, who in the most comprehensive manner, in his little paper, "Ueber progressive Bulbarparalyse," had endeavoured to make good this hypothesis, positively predicted that the next minute anatomical examination would discover the source of the affection in the medulla oblongata. He himself had not made an examination in his case of bulbar paralysis of this organ, which certainly differed in various ways from the typical form of the disease, and had contented himself with the demonstration of a degenerative atrophy of its motor nerve-roots; it was only after the sectio cadaveris that this view of the central and bulbar nature of the malady occurred to him. In a case of Schulz's the anatomical examination of the medulla oblongata failed in consequence of the preparation becoming spoilt; in this instance, in addition to atheroma of the basilar artery, Wedl was able to demonstrate only indefinite accumulations of granular cells in the medulla oblongata. Wachsmuth's prediction has been since that time fulfilled. Minute anatomical examinations have justified the name of progressive bulbar paralysis, and established the fact that the affection emanates from the motor gray regions of the bulbus medullæ. It is certainly true that this anatomical demonstration is by no means so easy a thing as it may appear to some of you.

As a matter of course, but little has been gained by the macroscopic reports. Only in exceptional cases was the bulbar nature of the disease made manifest to the unaided eye, in consequence of a striking hardness and total or partial atrophy of the medulla oblongata, as occurred in some cases of Trousseau, Leyden, and Wilks. Then, again, simple estimations of size and consistence by eye and finger are too much left to subjective measurement. The medulla oblongata itself and its olivary bodies are often still normal in structure, though exhibiting considerable induration. Conversely, as we are taught by the examination of our patient, the gardener's wife, to whom we have often referred, when there is to all appearances an entirely normal condition of the whole spinal marrow, enormous alterations of structure may nevertheless exist. In this case, even in the hypoglossi, which in so many instances were found to be distinctly atrophied and altered in colour, no irregularity of size or colour could be detected when a comparison was made with normal nerves of the same kind; for all that, their nerve-tubules were atrophied and affected with fatty degeneration; the circumference, however, of the nerve-trunks had not thereby been diminished, inasmuch as a soft, fleecy, fibrillary tissue occupied the place of the nerve-tubes. And even where atrophy and induration of the medulla oblongata can be detected without the microscope, it is only by the use of this instrument that we can obtain exact explanations with reference to the affected portions of this complicated organ, which is made up of so many parts endowed with so great diversities of function.

The microscopic examination, however, Gentlemen, is a troublesome business, and one which takes up much time, and which presupposes a complete familiarity with the methods of preparation in this special domain of anatomical art. In this case it is not sufficient to examine fresh preparations, for we can often only discover cells with fat-granules, and often, indeed, no abnormality whatever, in cases where, after the medulla oblongata or spinal cord has been properly hardened, a serious amount of degeneration has been brought to light. Among the materials for the hardening process, the chromic acid and chromate of potash have been proved especially to be valuable. They render the nerve-substance not only very hard for fine sections, they also excellently preserve the elements of the tissue, and particularly the delicate ganglion-cells and their prolongations. If the sections be rendered transparent by means of proper agents, Canada balsam, glycerine, very dilute solutions of soda, and the like, and then some colouring materials be made use of which have a peculiar affinity for this or that element of the tissues without injuring the structure of the parts—carmine, for example—we are able to bring into view, fully and sharply, nerve-tubes, ganglion-cells, and the substance cementing together (Kittsubstanz) these nervous elements, Virchow's neuroglia, and also the blood-vessels, and to distinguish them one from the other. In the cases which have advanced to degenerative myelitis, with proliferation of connective substance, there, moreover, the chromic preparations, by imparting a different colour to the normal and degenerated portions, are invaluable guides in enabling us to separate at a glance, even macroscopically, the healthy and diseased structures. The healthy portion becomes of a tawny, yellow colour, that which is the seat of the degeneration remains clear. If the section be then coloured with carmine, a reverse condition of things is exhibited, the clear degenerated parts now appear of a dull red colour, while those already coloured by the chromic acid admit the red colouring material with greater difficulty. The carmine has, in addition, the useful property of causing the ganglion-cells and their prolongations to stand out red in relief, while the nerve-tubes remain uncoloured, and form a strong contrast to the red connective substance. You perceive what great demands investigations of this kind make upon the patience, the dexterity, and the experience of the inquirer. Several weeks are required for a satisfactory hardening by means of the chromic preparations. Only recently W. Müller, of Jena, objected to such an authority as Cohnheim, and suggested that although he, in a case of so-called lipomatous muscular hypertrophy, had found nothing abnormal in the spinal cord, the reason for this possibly was, that he had allowed the parts to remain in the hardening solution for seventeen days only. Even the most skilful anatomist requires from two to three months to bring to a conclusion minute investigations of such cases as we are now considering.

Only during the last four years have we obtained possession of a number of reports of autopsies, eight in all, which have thus furnished us with more exact data concerning the anatomical changes in the medulla oblongata and spinal cord in the affection before us. Two of these we owe to Leyden, two cases complicated with progressive muscular atrophy were published by Charcot, Joffroy, and Gombault (in Charcot's section), a fifth we owe to Charcot, a sixth to Duchenne and Joffroy, seventh to the American, Hun, and an eighth was examined by my friend R. Maier. Maier examined the case of the gardener's wife, so often referred to, who died in my clinical wards in June, 1870.

These eight examinations afford us already a tolerably satisfactory view into the anatomical basis of the disease; both into the central region whence it starts, and into the morbid process which induces it. The question therefore before us always concerns an affection of the motor nuclei of the rhombic fossa, leading to a degenerative atrophy of the ganglion-cells of these nuclei. Sometimes, as in the two cases of Charcot and Duchenne-Joffroy, there is nothing of an abnormal kind to be found in the medulla oblongata, with the exception that the vessels of the gray nuclear substance are considerably injected. Sometimes, as in all the other cases, there is a chronic myelitis, going on to sclerosis. This, in all the cases except Gombault's, attacked the motor nuclei at the same time as the other motor regions, particularly the anterior pyramids, only in exceptional cases the olivary bodies; and never, up to the present time, the restiform bodies and their gray substance. In Gombault's case, the nuclei exhibited pigment atrophy of the cells, without change in the neuroglia. There was, however, marked sclerosis of the pyramids. We may assume that the atrophy of the nerve-cells in both the first-mentioned cases of Charcot and Duchenne-Joffroy had an independent origin, and that in the others it may have been brought about by the myelitis tending to sclerosis. At any rate, the destruction of the ganglion-cells in the motor nuclei must be regarded as the essential element of the morbid process, which lies at the root of the progressive bulbar paralysis, for all accounts are to this effect.

The degeneration of the ganglion-cells is presented to us, in quite a predominant manner, as yellow pigment atrophy. Their colour becomes darker, more yellow, often intense yellow, yellowish brown, or reddish yellow; they are sometimes diminished in size, and their prolongations become indistinct or quite disappear. In those which are less altered, the nucleus can still often be very distinctly recognised as a large, delicate object, the molecular protoplasm is made turbid. By the side of smaller granules, larger brown grains, and clusters of grains like mulberry-shaped cells, make their appearance. These are distinctly raised from the still remaining cell-contents and cell-nucleus. Other cells are quite full of these granules. The cell-nucleus has shrivelled up or has disappeared. This was observed by Maier in one case; and according to Charcot's statement, nothing finally remains of the degenerated cell but yellow grains separately dispersed or clustered together.

The form of myelitis tending to sclerosis, as it was found by Maier in one case, is entirely in accordance, in essential particulars, with Leyden's statements. In the fresh medullary substance there were countless conglomerate fat-granules, sometimes separate, sometimes in masses, covering the field of view. On preparations which had been hardened, the increase of the neuroglia was shown by a great extension of the interspaces between the nerve-fibres. The connective substance was sometimes more granular in appearance, sometimes filamentous, the filaments being either delicate or firm. The walls of the vessels were thickened, and had a broad sheath of connective tissue, in which there were often to be found fat-granules and brilliant disciform bodies, offering great resistance to chemical agents. Similar smaller granules appeared also in the thickened walls of the vessels, especially in the central gray masses, and the lumen of the vessels was then narrowed to a remarkable degree. Where the reticulated cellular tissue was still preserved, there were sometimes found large stelliform formations, like the corpuscles of the cornea (probably enlarged neuroglia-cells), with distinct prolongations, which proved themselves to be hollow by exhibiting albuminous granular contents, while the body of the cell itself often contained other cell-formations. The nerve-fibres, considerably reduced in number, were sometimes narrowed, their medullary contents in a state of granular fatty degeneration, the axis cylinders not seldom remarkably thickened; sometimes, and especially in those instances in which the interstitial tissue had become diffused and filamentous, the nerve-fibres appeared no longer granular, but pale, quite destitute of medullary substance; the axis cylinders were also paler, diminished in size, or had disappeared altogether.

Let us now turn our attention somewhat more minutely to the topography of these degenerative processes in the medulla oblongata. Of the gray medullary masses, which here correspond to the anterior columns of the spinal cord, and which, as I said, always exhibited alterations, the nucleus of the hypoglossus was always the one most affected by degeneration. Charcot and Joffroy convinced themselves, in the case which was complicated with progressive muscular atrophy, by comparison with Clarke's original preparations of normal hypoglossal nuclei, that the number of ganglion-cells in the atrophied nucleus was only one-tenth or one-twelfth of what it should have been. Degeneration has, moreover, constantly been found in the nuclei of the accessory; as a rule also in the nucleus of the facial, and by Duchenne in the nucleus of the motor portion of the fifth, while the externally situated nuclear masses, corresponding more to the posterior cornua of the spinal cord, the nuclei of the auditory, and the so-called sensitive nucleus of the fifth, and also, according to the positive assertion of Charcot and Duchenne-Joffroy, the nucleus of the glosso-pharyngeus, have appeared intact. Only Charcot found in the so-called sensitive nucleus of the fifth a small number of cells in a state of degeneration. The very complete destruction of the facial muscles in Hun's case, with preservation of the function of the muscles closing the eye, was explained by Hun by the statement that the roots of the nerve-twigs of these muscles had their ganglionic origin, not in the nucleus, but elsewhere. Leyden and Maier demonstrated a narrowing of the intra-medullary filamentary strands of the hypoglossus and vagus, accessory and facial. They were found to be normal by Gombault.

The olivary bodies, to which Wachsmuth, out of regard for the hypothesis of Schrœder van der Kolk, was inclined to impute so great a part in connection with this disease, appear, only in exceptional cases, to be involved in the process of degeneration. Leyden, in his last case, and the French observers, found them to be quite normal. In his earlier case, Leyden had found that the central substance was affected. Maier was the only one who observed very considerable degeneration in the central and in the wavy portions. It is, therefore, not necessary that there should be any degeneration of the olivary bodies for the production of progressive paralysis of deglutition and speech. The degeneration of the nuclei near the raphe is quite sufficient. It is certainly true that these negative statements do not contradict the hypothesis of the olivary bodies serving as the centre of co-ordination for articulation and deglutition, for in the disease under our notice we are not dealing with an ataxy, but with a real paralysis. For that assumption, comparative is far more fatal than pathological anatomy. The parrot, and other animals able to imitate articulate speech, have either no olivary bodies at all, or only rudimentary traces, while in the seal, whose eloquence has never as yet been manifested, they are perfectly enormous in size (Clarke).

Whilst the olivary bodies have been at least several times found in a state of degeneration, in all cases up to the present time the restiform bodies, with their gray substance, which proceed from the posterior strands of the spinal cord, have been found free from disease. The strands of the pyramid, on the other hand, together with the zonal layers and the commissural fibres, were found by Leyden in two cases, by Maier in a case of mine, and by Gombault in a case complicated with progressive muscular atrophy, together with the reticular substance, to be in a complete state of sclerotic degeneration. In Leyden's and Maier's cases the degeneration had its seat in the pyramidal filaments of the pons; in our case it was even continued through these into the commencement of the inferior portion of the crus cerebri. It showed itself here, however, only as a simple fatty metamorphosis of the interstitial tissue, with deposit of granular masses.

As you have heard, it happens in the course of bulbar paralysis that sometimes sooner, sometimes later, paralysis invades spinal muscular groups, and such muscles often become atrophied; and we cannot therefore be surprised that in all cases in which, up to the present time, the spinal cord has been minutely examined, degenerative changes similar to those occurring in the bulbus medullæ have been found to exist. Sometimes all that has been demonstrated has been pigment atrophy of the ganglion-cells in the anterior cornua, with marked vascular development (Charcot and Duchenne-Joffroy); sometimes a myelitic tendency to sclerosis was found in the anterior cornua, anterior and lateral strands (Leyden, Hun, Maier), together with this atrophy of the ganglion-cells. This latter affection attacked in the most intense degree the posterior parts of the lateral strands, and the inner portions of the anterior strands lying close to the anterior fissure; a circumstance that reminds one of the predilection of even the secondary degenerations of the spinal cord in cases of "apoplectic" deposits in the brain for precisely these portions of the anterior and lateral strands. No alterations have been, up to the present time, found in the posterior cornua and posterior strands; in our case, only, Maier noticed that in the cervical portion single granular cells penetrated into the lateral boundaries of the posterior cornua. In both the cases in which the bulbar paralysis was accompanied by a progressive muscular atrophy, there was found to exist myelitis tending to sclerosis of the anterior and lateral strands, with pigment atrophy of the ganglion-cells of the anterior cornua; and Gombault observed these ganglion-cells of the anterior cornua to be reduced to one-fifth in the cervical, and to one-half in the lumbar portion of the cord.

I have already referred to the frequency with which the roots of the motor bulbar nerves appear atrophied, even to the naked eye. Most commonly the roots of the hypoglossus were found to be attenuated and transformed, indeed, into such fine threads that they could scarcely be discovered. Also the vagi, accessory and facial, were more frequently noticed to be thinner than natural. Leyden found this to be the case with the glossopharyngei; Hun, indeed, with one-fifth pair and the abduceus, while the other fifth nerve was thickened. The same statements frequently apply to the anterior spinal roots, sometimes only the cervical ones being affected; sometimes, also, those which have come off lower down from the spinal cord. Even if the nerve-roots have preserved their size, there may yet be, as our case teaches us, a considerable amount of atrophy of the nerve-tubes, partly in the form of a simple narrowing, partly as a fatty degeneration, and the gaps may be filled up by an abundant wavy connective tissue, not abounding in nuclei. In pure pigment atrophy of the ganglion-cells, Charcot was able to recognise, with the microscope, nothing but a fine granulation of the nerve-tubules at the roots of the nerves. Where fatty atrophy of these had occurred, as is commonly the case, it diminished towards the periphery. On the finest ramifications of the hypoglossus in the interior of the tongue, this could sometimes only be demonstrated on single filaments; sometimes the nerve-twigs were quite normal. In one of my cases Maier found, here and there, simply a narrowing of the nerve-tubules.

With reference, in conclusion, to the paralysed muscular regions, we notice the tongue, the lips, and the muscles of the throat, trunk, and extremities sometimes diminished in size; at the most, corresponding to the general emaciation; sometimes decidedly atrophied. We must not fall into the same error as Duchenne, and conclude definitely, that because the size was preserved the muscles retained their nutritional integrity, not even if the faradic excitability is still maintained. The nutritional integrity may remain, as Trousseau and Duménil found in the tongue, and as Maier demonstrated microscopically on the orbicularis and on many muscles of the extremities which had become paralysed; it may, however, be wanting. This was particularly proved by the oft referred-to case of Charcot's. Although in his case of pigment atrophy of the bulbar nuclear-cells no important diminution of the size of the tongue could be perceived only a few days before death, the flesh at the tip thereof was yet in a condition of considerable fatty degeneration, and its fibres atrophied, while the sarcolemma and perimysium exhibited an abundant growth of nuclei. The so-called pseudo-muscular hypertrophy must be carefully borne in mind. In this affection, which certainly is more often of central origin, and proceeds from an atrophy of the spinal anterior cornua and their ganglion-cells, an advanced atrophy of the muscular tubes may, as is well known, lurk behind the hypertrophy of the interfibrillary adipose tissue; and there are all kinds of transition, from increase to diminution of volume. The electrical excitability in these cases is diminished only in high degrees of atrophy of the muscular tubes. I may here certainly remind you of this form of paralysis, for even in progressive bulbar paralysis an increase of the adipose tissue between the atrophied muscular fibrillæ of the tongue has frequently been met with, and I should not be surprised if, in future, cases associated with evident hypertrophy of the tongue should come under observation.

Of great importance is the fact, to which I must hereafter refer when I come to establish the relationship of this affection to progressive muscular atrophy, that in the same individual, muscles in juxtaposition, and entire groups of muscles which were paralysed during life, may, when microscopically examined, appear partly intact, partly degenerated, and atrophied in the most dissimilar degrees of extent and intensity. Very instructive, indeed, in this respect was our observation in the case of the gardener's wife, who finally became affected with paralysis of almost the whole of her body, with anterior degenerative myelitis. In this case, for example, there was great atrophy of the tongue; the orbicularis oris was unaffected; the superficial muscles of the neck were greatly degenerated and atrophied, the deep ones either not at all or only a little affected; the degeneration was very great in the laryngeal muscles and only slight in those of the pharynx; considerable in those of the trunk and leg, while those of the thigh were scarcely, if at all, affected. A similar state of things was found by Charcot, who, in the case above cited, found in several muscles these nutritional alterations confined to separate fasciculi and fibres; he noticed others, such as the muscles of the left shoulder, to be greatly atrophied and yellow; others, again, simply emaciated.

The actual atrophy seldom goes on to total withering of the muscle, and, as a rule, occurs only in separate fasciculi and fibres in unequal proportions. It appears sometimes as a simple diminution of size, sometimes as a fatty, sometimes as a waxy degeneration of the muscular tubes; all these forms of degeneration may be met with in the same muscle. The waxy degeneration existed in the case examined by Charcot-Joffroy, and in two of my cases to a great extent, along with the two other forms. There has been some desire to regard this waxy degeneration of Zenker's as the product of a post-mortem process; but I can assure you that Maier and I, in the year 1865, examined a small piece of the flesh which had been just recently excised from the sural muscles of a man, these being quite paralysed and unexcitable by means of electricity, and the patient having been formerly the subject of what I now conceive to be myelitis anterior, and that this portion so excised exhibited the highest degree of waxy degeneration.* The interfibrillary connective tissue appears often, but not always, increased in this atrophy; its nuclei sometimes also increased.

The possession of these facts, Gentlemen, places us, as I think, now in a position to express ourselves definitely with regard to the relationships of progressive bulbar paralysis to progressive muscular atrophy.

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* Conf. Deutches Archiv. f. klin. Med., vol. i. p. 512.

How intimate these relationships are, follows from the fact that both forms of paralysis are so extremely often associated, inasmuch as they either appear together at the very beginning, or later on in the case the one is joined to the other. Not only does the bulbar paralysis often play an important part in the terminal stages of progressive muscular atrophy, but it has also the most marked tendency, in those cases where it occurs primarily, to extend itself to the spinal cord, and to combine itself with the clinical type of a progressive muscular atrophy and paralysis. It is very significant that in all more minute autopsies up to the present time, without exception, the same anatomical changes have been found in the spinal cord as in the bulbus medullæ, and notably, also, only in the motor portions.

It is true that if we could, with Duchenne, consider as real instances of bulbar paralysis only such cases in which the tongue is paralysed, but not atrophied, the question whether progressive bulbar paralysis and muscular atrophy are cognate affections, would be immediately decided in a negative sense. You have, however, already become acquainted with numerous facts which cause this assumption of Duchenne's to appear to us purely arbitrary. You have heard how gradually in progressive bulbar paralysis the changes go on from simple paralysis to degenerative atrophy of the muscles; how impossible it often is on the living subject to decide whether the tongue, to all appearances of normal thickness, is normal or already atrophied in its fleshy substance; finally, how, besides muscles and groups of muscles which are simply paralysed, others are found atrophied in the same individual. Exactly the same thing again occurs, as Benedikt (1863) was the first to point out, in progressive muscular atrophy; in this case also, muscles which are simply paralysed are often found along with those in a state of atrophy. This fact is not merely a clinical one; it has also been determined by means of precise anatomical examinations. Hayem has, for example, in 1869, reported a case, than which nothing could be more demonstrative. In a man with progressive muscular atrophy, a paralysis of the diaphragm was shown to exist in the year 1867. Later on the existence of this paralysis was confirmed by Duchenne himself, and he found, in addition, the serratus magnus, sterno-cleidomastoid, and the scaleni paralysed, and, as he thought, atrophied. At the autopsy Hayem found all these muscles attenuated to a degree corresponding with the general emaciation, but in other respects quite normal. There is, therefore, neither in the clinical nor in the anatomical conditions of the muscles, any essential dissimilarity between the progressive muscular atrophy and the progressive bulbar paralysis. Not merely, however, in the condition of the muscles, but also in the gradual and, at times, rapidly increasing progress of the development of the disease, and its marked tendency to an incessant advance, which is only in exceptional cases interrupted by quiescence, improvement, or recovery, and also with reference to the anatomical changes in the nervous system, we have manifestations of the existence of the most extraordinary resemblance. The shorter duration, the greater danger of bulbar paralysis, are sufficiently explained by the fact of its point of origin being in the centrum vitæ, without being obliged to seek the reasons for this difference in the dangerous nature of the processes on which it depends.

Only since an improved method of preparation has enabled us to ascertain more minutely the finer changes of structure of the spinal cord has there been any illumination of the obscurity in which, up to that time, progressive muscular atrophy was enveloped. The history thereof shows us that a precisely similar change has taken place in the views held as to the nature of this disease as in the case of progressive bulbar paralysis. Misled by the strange account of coarser macroscopic anatomical changes in the spinal cord, while muscles and anterior spinal nerve-roots were far more often found to be degenerated, the majority of observers formerly inclined to the view that progressive muscular atrophy depended essentially upon a myopathic affection, or an original degeneration of the anterior spinal roots. But the more minute clinical study of the disease led, in an ever-increasing circle, to the conviction that we here had to do with a central spinal affection. I call to mind only the numerous reasons which Benedikt, Bergmann, Rosenthal, and others have advanced in favour of this view: the derangements of sensibility which manifest themselves now and then at the commencement and as the disease advances, especially the pains in the back and extremities, the symptoms of a spastic kind which sometimes occur, as well as the augmentation of reflex action, the nutritional derangements in the bones and joints, these being of rare occurrence, but particularly the progress by fits and starts, the predilection for a symmetrical advance, and the frequent combination with other spinal and central lesions in general—for example, tabes, bulbar paralysis, and so on. Added to these, the ever accumulating positive anatomical statements which the advanced technical skill of the more recent observers has obtained for us in cases where the old methods were powerless.

We have truly important facts to consider, when a man like Lockhart Clarke, so remarkably conversant with the histology and pathological anatomy of the spinal cord, asserts (1868) that in not less than eight cases of muscular atrophy, several of which presented the particular type of the progressive disease, he found the most unequivocal lesions of the gray substance of the spinal cord and its nerve-roots in the form of a degenerative atrophy. But not only in England, also in France and Germany, with every year there is an increase in the number of minute investigations, which place beyond doubt the fact of a causal connection between wasting of muscle and atrophy of the gray substance of the spinal cord, and particularly of the large cells of the anterior cornua. Charcot especially has the credit of having recognised the nutritional importance of these cells, and, in common with his pupils, of having demonstrated in numerous cases their degenerative shrivelling in progressive muscular atrophy, in the spinal paralysis of children, and other forms of spinal paralyses leading to muscular atrophy. Recently the same spinal and ganglionic point of departure in the anterior cornua has been, by German inquirers, discovered for the so-called pseudo-hypertrophy, or lipomatous hypertrophy of the muscles (Barth, W. Müller). Charcot has good reason for assigning almost dogmatic importance to the fact, that in these atrophies the region of the altered gray portions of the spinal marrow corresponds with the points of exit of the motor nerve-roots which supply the atrophied muscles with nerve-twigs; and W. Müller also, who states that the number of muscles in a condition of degeneration keeps pace in an approximate degree with the atrophy of the anterior ganglion-cells. It is, moreover, a fact of great importance, that up to the present time there exists no minutely investigated case in which a degeneration strictly confined to the white strands of the spinal cord had led to decided muscular atrophy, whilst there is no want of observations of cases in which also muscular atrophies occur in fasciated atrophy of the posterior strands with the form of tabes dorsalis, or fasciated atrophy of the lateral strands and disseminated sclerosis of the brain and spinal cord, with their characteristic forms of paralysis and contraction of the muscles; and, lastly, with the encroachment of the atrophy upon the anterior cornua. And, in conclusion, the investigations of Vulpian and Dickinson (Clarke), show that the loss of large masses of muscles, by amputation of entire limbs, never induces a degenerative atrophy of the ganglion-cells, but only, very slowly, a simple atrophy, moderate in amount, of the corresponding portions of the spinal cord; at all events no adequate diminution of the ganglion-cells attends the loss of the muscles.

The degenerative processes, which in progressive muscular atrophy have, up the present time, been demonstrated on the spinal cord, are manifold, and have nothing specific about them, nothing which belongs only to this disease. We meet here with the common gray degeneration, therefore with simple increase and consolidation of the connective tissue with fatty disintegration of the nerve-tubes, as it is found in tabes dorsalis so frequently upon the posterior strands, and again, as a consequence of the so-called apoplectic cerebral deposits on the posterior lateral strands, and in the disseminated sclerosis, as dispersed insular formations in the brain and spinal cord. At other times we have to deal with a chronic indurating myelitis with unmistakeable products of a great irritation of the elements of the tissue, as I have previously described it to you; a process which is found likewise occasionally in the similarly-named sclerosis of the spinal cord. In one case, Frommann ascertained that there was a red softening. Clarke found repeatedly that peculiar form of softening which was designated by him "granular disintegration;" and by Charcot, who likewise described it, "disintégration granulaire," that is, granular disintegration. In this "Clarke's softening," as I will tersely name it, in the first place the tissue surrounding the vessels becomes softer and more translucent. The medullary layer of the nerve-tubes crumples up into small fragments and globular flakes, which must not be confounded with the granular-cells, which are of less frequent occurrence. If the process further advances, the substance always becomes more transparent and fluid; finally, nerve and connective tissue are changed into a fine granular detritus, or a liquid holding fine granules in suspension. Fissures and canals are ultimately formed in the spinal cord, microscopic, or even visible to the naked eye, and these traverse the white strands, and also the columns of gray matter, frequently for a considerable distance. At the same time, there may be sclerotic thickenings and condensations of the blood-vessel and connective substance. Even this "Clarke's softening" does not occur only in progressive muscular atrophy, it has also been found in tetanus, chorea, and other affections. In conclusion, we are taught by Charcot's case of progressive muscular atrophy, which was associated with original bulbar paralysis, that even a pigmentary degeneration and atrophy of these elements, confined purely to the ganglion-cells of the anterior cornua, may originate the form of that affection. But even this pigment atrophy of the ganglion-cells is a process which can wear itself out on the most diverse central regions, as follows especially from Meschede's labours with reference to the fatty pigmentary atrophy of the ganglion-cells of the cortex of the cerebrum in the "Paralysie générale," and which also takes place in the spinal cord under the influence of numerous acute and chronic inflammatory processes.

In all these various processes there consequently appear to be two essential requisites for the production of the form of disease of the progressive muscular atrophy—1. That the great polyklonic ganglion-cells of the anterior cornua should become wasted; and 2. That this wasting process should extend itself in a definite manner to the anterior cornua. Such processes only are capable of producing that clinical type as have a natural tendency to creep on to the anterior cornua and the ganglion-cells in a scattered manner; to cause wasting, in the one sooner, in the other later, in the one more, in the other less completely, while yet other single cells, and whole groups of cells, remain exempt. The clinical course of the disease harmonises best with this ingenious hypothesis of Charcot. This explains to us better than any other the wonderfully fitful manner in which separate muscular fasciculi, muscles and groups of muscles, are attacked, while a certain preference for co-ordinate and symmetrically placed groups cannot be mistaken, and, at the same time, it enables us to solve the question why progressive muscular atrophy sometimes occurs as a simple affection, and sometimes associated in the most manifold way with tabes, paralyses, contractions, anæsthesia, and so on. All depends upon the place of origin of the degenerative process, and upon the regions of the spinal cord over which it spreads.

As little as the bulbar paralysis is the progressive muscular atrophy a peculiar kind of disease—it is only a form of disease—a knot of symptoms which develops itself, advances, and comes to an end in a determinate way. Without sharply defined demarcations, the typical form changes into others allied thereto; and it will frequently be a matter for dispute whether a spinal paralysis, connecting itself with muscular atrophy, can be thus or otherwise designated. Where we have not to deal with primary progressive degenerations of the anterior gray columns of the spinal marrow quite independent of other spinal lesions, but with those of a secondary nature, the decision may often be a very difficult matter. As we now know, traumatismus (Bastian, rupture of the spinal marrow), tumours (Sander), and hydromyelus (Gull, Schüppel, Grimm) may lead to the formation of the progressive muscular atrophy, or may present clinical features having a close resemblance thereto; the hydromyelus, indeed, may be less of a cause than a co-effect of a myelitis tending to sclerosis of the gray central substance by a kind of cirrhotic retraction thereof. The relationship is clearer in the cases in which tabes, diffuse chronic myelitis, multiple sclerosis, or the secondary degenerations of the posterior strands after cerebral apoplexy, are associated with the muscular atrophy.

If we now return to progressive bulbar paralysis, and ascertain which of the degenerative processes, leading in the spinal cord to progressive muscular atrophy, cause, in the medulla oblongata, that form of paralysis and atrophy, we find, up to the present time, besides the simple pigment atrophy of the ganglion-cells, that only those forms of degeneration which cause sclerosis are represented. Those processes leading to softening are, as it seems, of a too active character, and more rapidly destructive of larger connected tracts, and are more liable to produce apoplectiform than progressive bulbar paralysis. It follows, nevertheless, from Clarke's observations, that a terminal paralysis of the tongue may occur in progressive muscular atrophy with "Clarke's softening" of the spinal cord, and in a case of Gerhardt's, after an injury to the head, a secondary atrophy of the medulla oblongata, with the symptoms of the bulbar paralysis, and a fatal termination in one year, was associated with red centres of softening in the pons and cervical portion of the spinal cord.

The main result of these considerations may consequently be expressed as follows. The progressive paralysis of the tongue, and the progressive muscular atrophy, are forms of disease at the root of which lie manifold degenerative processes, either in the medulla oblongata or in the spinal cord; processes which agree in this, that on the one hand they have the tendency to extend gradually over the anterior gray medullary column of the spinal cord and the nerve-nuclei of the medulla oblongata corresponding thereto, and on the other hand to cause wasting of the ganglion-cells therein embedded, either dispersed separately or collected in groups. According as such a process primarily attacks a higher or lower region—the medulla oblongata, the cervical or lumbar enlargement—the forms of the disease vary without losing their typical character. Obscure forms and multifarious complications with different neuroses; tabes, contractions, hemi- and paraplegia, progressive paralysis of other cerebral nerves, and so forth, are results of previous, coetaneous, or subsequent extension of the same processes into other districts of the spinal cord, the medulla oblongata, or even more highly situated central regions.

We have, then, obtained the anatomical ground for a theory of the progressive bulbar paralysis and muscular atrophy at the same time; unfortunately, however, we are still in need of a satisfactory physiological conclusion.

If in these affections we were dealing only with muscular atrophy, to the intensity and extent of which the muscular weakness stood in direct proportion, the matter would be quite simple; we could then conceive of muscular atrophy and muscular paralysis running quite parallel with atrophy of the ganglion-cells. The perfectly typical classical cases of progressive muscular atrophy have, indeed, this character of the pure, nutritional myopathia, and they almost tempt one to regard the degree and extent of the muscular degeneration as a gauge of the force and extension of the same affection in the ganglia. But this formula is open to the objection, that in many cases the paralyses and muscular atrophy are not proportional to each other, and that the slight atrophy of the tongue, in the cases reported by Charcot and Duchenne-Joffroy, if, indeed, it existed at all in the latter instance, was in wonderful contrast to the great diminution of the hypoglossal nuclear cells. We have, therefore, a contradiction before us; on the one hand numerous anatomical accounts are in favour of a nutritional dependence of the muscles upon those ganglion-cells; on the other, the wasting of the ganglion-cells is not in direct proportion to that of the muscles.

To clear up this obscurity, there is need of still further exact clinical anatomical investigations, and still more so of physiological experiments. The experiments of Erb, Ziemssen, and Mantegazza, have made us well acquainted with the nutritional derangements which the muscle undergoes if its motor conducting channels are severed from their connection with the nearest excitory sources in the ganglion-cells of the anterior cornua; they correspond with those which occur in progressive muscular atrophy and bulbar paralysis. We know also, specially, with regard to the tongue, through Vulpian's experiments, that the transverse section of the hypoglossus causes nutritional alterations in the fleshy substance of the organ, similar to those seen with degeneration of the hypoglossal nuclei, while a transverse section of the lingual nerve has no such results. On the other hand we possess, so far as I know, no experimental work which has set before itself the task of ascertaining more exactly the influence of the medullary motor excitory sources upon the nutrition of the muscle. We know only, in a general way, that transverse section of the spinal cord may give rise to fatty degeneration of the muscles (Scherschewsky), and an experiment of Vulpian's proves that the intra-medullary destruction of the nerve-tubules of the facial and abducens oculi is able to cause atrophy of their muscles. We have, therefore, nothing but hypotheses to fill up for the present these blanks.

The opinion has formerly been held, that the nutritional derangements in the affections under our notice might, upon the whole, arise, not from medullary changes, but from lesions of the sympathetic, whose marginal strand was found atrophied in three cases of progressive muscular atrophy; once by Schneevogt, and twice by Jaccoud. At the same time, however, in all three cases there was atrophy of the anterior spinal nerve-roots, and, in Schneevogt's case, softening of the spinal cord. Inasmuch as, however, in the progressive muscular atrophy and bulbar paralysis, the sympathetic and its ganglia have repeatedly been found unchanged (Leyden, Hayem, Charcot-Joffroy, R. Maier, in my case), as likewise the spinal ganglia (Leyden), it is obvious that the medullary lesion is quite sufficient to produce the above-named affections, with their derangements of muscular nutrition.

Brown-Séquard and Charcot advance the opinion, that the effect of atrophy of the ganglion-cells may vary, according as it either is, or is not, connected with irritation. The pure atrophy (of these cells) implies only paralysis; the more the destruction is brought about by means of irritative processes, the more certainly and rapidly does atrophy of the muscle ensue. Duchenne and Joffroy conceive the matter to be more simple. They consider themselves warranted in the assumption that the ganglion-cells in the anterior cornua and motor bulbar nuclei are of a twofold kind—motor and nutritional. Where the typical form of progressive muscular atrophy occurs, there we have to deal with a lesion confined to the nutritional cells, where, as in the cases of progressive paralysis of the tongue without wasting, the paralysis advances without atrophy, we have a lesion of the motor cells only. It is easier to find out the weak points in these hypotheses than to offer a better explanation in their stead.

An attempt has been made to extend the scheme of progressive paralysis of the bulbar nuclei so as to include the progressive paralysis of the cerebral nerves (Benedikt). The plausible assumption lies at the root of this attempt, that the degenerative neuritis may attack nuclei more anteriorly situated beyond the boundaries of the lozenge-shaped depression, and particularly that paralysis of the motores oculi twigs may usher in a progressive bulbar paralyses, or may accompany this affection. A case of older date, by Romberg and Davaine, was thus explained by Wachsmuth. The affection, however, in a man thirty-four years of age, lasted for twelve years, and appeared to become stationary; he suffered from facial diplegia, paralysis of the tongue, palate, and superior recti muscles of the eyes.

Benedikt has just recently described, under the above title, not less than seventeen cases observed in four years, all of which he refers to "diffused neuritis of the cerebral nerves, and of their nuclei." We find here, so far as the fragmentary reports of the diseases allow of a judgment being formed, by the side of a few progressive bulbar paralyses of the usual form, complicated types of disease of all kinds. Multifarious initial cerebral symptoms, melancholia, loss of consciousness, with convulsions, muscular contractions, and so forth, ushering in sometimes the acute occurrence, sometimes the chronic advance, of paralysis, on one or both sides, of the motores oculi, abducentes, and upper facial branches, either with paralysis of deglutition or of speech, or with both at the same time. Paralysis of the phrenic and other spinal nerves occurred in addition, and now and then there were clonic and chorea-like convulsions of separate muscular districts, even also ageustia and anæsthesia. Muscular atrophy, on the contrary, appears to have been found by Benedikt, in his seventeen cases, only once on one hand.

Unfortunately, we look in vain for one post-mortem report which would justify us in summarily mixing up together all these forms of acute and chronic progressive paralysis of the cerebral, bulbar, and spinal nerves with Duchenne's paralysis. Our scruples must increase when we learn that exactly in five cases, which deviated very considerably from the ordinary type of the disease, Benedikt attained the cure of the disease by galvanism, while those which to the greatest extent agreed therewith had a fatal issue, and among them, one which came very early under treatment. I think it desirable, under such circumstances, not to allow, for the present, the progressive bulbar paralysis to be confounded with Benedikt's progressive paralysis of the cerebral nerves. On such difficult paths of investigation we cannot be sufficiently cautious in separating the certain from the doubtful grounds for our opinions.

My advice certainly is that we should at present refrain from explaining anatomically those cases where we have before us, not a paralysis of the motor oculi, but total hemiplegia or diplegia facialis with lagophthalmos, as was the case, for example, in Wachsmuth's patient, and that we should wait for explanations from future post-mortem reports. Where, however, deafness, ageustia, neuralgia of the fifth pair and anæsthesia, clonic and tonic spasms complicate the case, or where the paralysed muscles rapidly lose their faradic excitability, or the course of the disease differs remarkably from the ordinary one, either as regards its rapidity or the unusual succession of the symptoms, a double amount of care is necessary. There is, at the present, no lack of errors of diagnosis to warn us against precipitancy.

Erwin Bälz has recently (1872) published a case, from the Leipsic polyclinic, of enchondroma of the base of the skull, with compression and hæmorrhage of the medulla oblongata, besides compression of numerous bulbar nerve-roots. This case was looked upon in the clinical wards as an ordinary one of bulbar paralysis. The patient had, in this case, been under observation for too short a time, and it had not been understood that the malady commenced with neuralgic pains of one trigeminal nerve, and clonic spasms within the range of distribution of the facial and hypoglossal; these pains continued until the patient died, having at last assumed the character of anæsthesia dolorosa. It is remarkable that, while the patient was under observation in the clinical wards, of the muscles of the face, just as occurs in true bulbar paralysis, only the orbicularis oris, possibly also the muscles of the chin, was observed to be paralysed. The tongue was considerably atrophied.

Recently, Gentlemen, when making a post-mortem examination of the body of an old man, we accidentally discovered numerous warty excrescences of the size of a pea on the choroid plexus of the fourth ventricle, which grew from its roof towards the rhombic fossa. If such tumours attained such a size as to cause compression of this rhombic fossa, they might occasion symptoms resembling those of a progressive paralysis of the bulbar nuclei. Voisin, indeed, noticed a form of bulbar paralysis having its origin in such an epithelioma of the visceral layer of the arachnoid, which compressed nearly all the bulbar nerves, but the paralysis of deglutition and speech occurred suddenly, taste and hearing were destroyed, and respiration impeded. The power of moving the limbs remained unaltered.

In addition to tumours, syphilis, especially, often appears to be the exciting cause of variously complicated forms of bulbar paralysis, which should not be confounded with Duchenne's disease. Such patients may be cured by iodide of potassium, as is proved by two cases of Cheadle and Silver. The latter authority wishes to recognise this form of labio-glosso-laryngeal paralysis as retrogressive, and to distinguish it from the progressive form.

In addition, you must guard against a too rapid diagnosis of progressive bulbar paralysis in dealing with hysterical derangements of deglutition and speech. There are two hysterical cases among the presumably cured patients treated by Benedikt. Generally speaking, too, you must not forget that paralyses of deglutition and articulation, of varying grades, are extremely frequent phenomena in very different acute and chronic affections of the nervous system. We can legitimately speak of a progressive paralysis of the bulbar nuclei, only when the disease presents itself in the peculiar form first depicted in so masterly a manner by Duchenne and Trousseau.

Notwithstanding the slender results, up to the present time, of our therapeutical efforts in this disease, we will not despair of better success in time to come. Inasmuch as the repeated application of galvanism has caused considerable improvement, though unfortunately only of a temporary kind, and has caused the morbid process evidently for the time to retrograde and stand still, it may be also possible to bring about a permanent cure, either by the more early and continuous application of the methods at present in use, or by employing others which may be subsequently discovered. As far as we have gone at present, the patients almost always came under medical treatment only after many months had elapsed, when a great number of the ganglion-cells had been destroyed. Physicians, also, have had too slight a knowledge of the disease; mistakes have occurred in several cases, and I myself have met with such, in which carcinoma of the œsophagus was suspected, or which was supposed to be a slight diphtheritic paralysis of the palate, which was confidently left to the vis medicatrix naturæ. We may now venture to hope that the disease, into the nature of which, moreover, an insight has only just been obtained, will be earlier recognised and more energetically treated.

As a matter of course, the patients should be removed, as soon as possible after the first appearance of suspicious symptoms, from all noxious influences, such as may be due to their avocations, to the neighbourhood in which they live, or to external circumstances in general. The methodical nursing of a good hospital offers the most favourable conditions for the poorer class of patients.

In cases where the initial pains in the head and neck point to the myelitic nature of the morbid process, I shall in future, when the patients come early under treatment, prescribe the repeated application of dry cupping-glasses to the nape of the neck every five days, and in full-blooded, strong persons, shall order blood to be abstracted at the first cupping. Then I should permit the cautious tentative use of the shower douche, and guided by the result of this, even the jet douche for strong persons. These remedies have, moreover, been found useful in tabes dorsalis and chronic diffuse myelitis. All articles of food of a stimulant nature must be avoided during the first months of the disease; later on some good wine may be recommended as beneficial for debilitated persons. In order to facilitate mastication and deglutition the victuals should be made soft, and cut up as small as possible before being administered.

With regard to medicines, silver, administered early, is deserving of the most confidence, for there is no doubt that it now and then acts favourably upon chronic degenerative processes of the spinal marrow. Gold might also be tried. I and others saw no benefit result from the use of iodide of iron in cases certainly already far advanced. There are objections to phosphorus, a medicine which the French physicians have very freely administered in this and other chronic degenerations of the nervous centres during the last few years. The patients might have been spared the evil results that followed, if attention had been paid to the more recent experiences of the frightful degenerations which numerous organs and tissues of the body undergo under the influence of small doses of phosphorus. Strychnia also has been found to be useless, and the bromide of potassium can only do mischief.

The local faradisation of the palate, the tongue, and the hypoglossal nerves, as was practised by Duchenne, causes, as my first case convinced me, an improvement in the deglutition and articulation, but only of a transient character.

Galvanisation, applied in various ways, has been found more efficacious by Gerhard, Schulz, Benedikt, Rosenthal, and others. I myself, in the cases of the priest and the gardener's wife, applied galvanism daily for some weeks, sending strong currents and changing their direction, first through the neck, and afterwards through the entire vertebral column; and in the same sitting also, currents in alternate directions from the neck and hypoglossus to the tongue, and in the case of the priest, who was paralysed in both arms, I sent currents from the neck through the nerves of the arms. The results at first were strikingly marked, especially in the case of the priest, although the paralysis had, in his case, existed for eight months, and was far advanced. Before the first sitting the patient could scarcely raise the arms above the horizontal line; immediately afterwards, and perhaps for an hour subsequently, he could place them vertically; in like manner deglutition immediately became easier, and the flow of saliva less. After a few sittings he could protrude beyond the lips the atrophied tongue, which before that could only be got as far as between the teeth; his food went less frequently in the wrong direction; he was able again to put on his hat, to open and shut doors, to pour out water for himself, and to bring the glass to his mouth, even to eat slowly with knife and fork, all of which actions he had no longer been able to perform. The speech, which had become quite unintelligible, stammering, and nasal in tone, was more distinct, at least in the morning after sleep. Strong currents, from thirty to forty Siemens' elements, to the back and arms, were very well borne by this patient, no pain being caused. After four to five weeks the improvement no longer went on. Electricity was now only used every two, and subsequently every five, days. Things now remained stationary for eight weeks, although the so-styled galvanisation of the sympathetic on both sides of the neck was still persevered in. The patient left Freiburg, and the paralysis soon again increased. To the same effect are most of the reports of those physicians who have used galvanism. As to the other methods, there is that of Schulz, who induces a number of movements of swallowing, fifteen to twenty in a sitting, by exciting one or other of the hypoglossal nerves, and that of Benedikt, who sends for a minute, through the mastoid processes, currents whose direction is changed at the half-minute.

If the galvanisation is no longer able to reduce the dysphagia to a degree compatible with life, the patient must be artificially fed with the œsophagus tube.

In a remarkable case, concerning which Benedikt made a report to the Société de Médecine of Paris, on the 17th May, 1872, tracheotomy was performed, by Fauvel's advice, as a precautionary measure against dangerous attacks of dyspnœa of a laryngeal character, occurring repeatedly day after day, and the tube was suffered to remain in. The attacks threatening suffocation ceased, the paralysis of deglutition and articulation advanced. The cause of these attacks, threatening suffocation, was not discovered. They were associated with aphonia and a sensation of constriction in the larynx. With the laryngoscope, Fauvel could discover nothing particular. The closure of the trachea above, towards the larynx, by a canula which had been inserted and allowed to remain after tracheotomy, might, in many cases, assist to prolong life; for when the larynx could not be closed naturally, the canula would protect the air-passages from the intrusion of anything contained in the mouth.

THE TREATMENT

OF

SIMPLE ULCER OF THE STOMACH.

BY

PROF. H. ZIEMSSEN,

OF ERLANGEN.

GENTLEMEN,—The pathology of the simple ulcer of the stomach has for some ten or twenty years again and again been made the subject of careful studies, by which our knowledge has been essentially increased, and a deeper insight afforded us into the origin and course of the ulcerative process. A similar assertion cannot be made with reference to the therapeutics of ulcer of the stomach; but little has been gained in this respect by the advance of pathology. We must confess, with reference to treatment, that as regards the most important points we have made no progress since Cruveilhier and Autunrieth. Notwithstanding this, however, I believe that our present knowledge of the pathologico-physiological processes in ulcer of the stomach, coupled with our clinical experience, enables us to comprehend more clearly the indications, and to lay down a rationally empirical line of treatment corresponding thereto. I might venture to assert that there is scarcely any other affection in which a like clearness of the object to be attained, and of the steps to be taken, is combined with the same amount of certainty of obtaining the desired results.

Before we turn to the consideration of the pathologico-physiological processes, which are the objects of our treatment, I will, very briefly, remind you of those points to which the present state of our knowledge teaches us to attribute predominant importance in the origination of ulcer of the stomach.

First and foremost are the changes in the gastric blood-vessels, which reduce or interrupt the circulation within a circumscribed area of the coats of the stomach, and thereby expose the portions affected to the corroding influence of the gastric juice, and to digestion by the juice itself. In ulcers of recent occurrence, Virchow, Rokitansky, Merkel, and others, have shown that the lumen of small arteries may be completely occluded by emboli or thrombosis, by fatty, atheromatous, and amyloid degeneration of the walls of the vessels; and this has been confirmed by the experiments of Pavy, who caused ulcer of stomach in animals, by placing ligatures on the branches of the gastric arteries. It is also in the highest degree probable, that disturbances of the circulation of another kind, *e.g.* intense inflammatory or collateral hyperæmia, perhaps in consequence of an extensive burn of the skin, or great venous stases, arising from mechanical obstacles in the course of the portal vein, may induce serious derangements of nutrition in circumscribed regions of the walls of the stomach, while, in consequence of the rupture of the smallest vessels, there is a hæmorrhagic infiltration of the mucous membrane. The nutrition of a portion of the mucous membrane may have suffered in consequence of the interruption to the supply of blood, or of hæmorrhagic infiltration: in both cases the affected part of the mucous membrane is exposed to the corrosive influence of the gastric juice, or to the acrid, acid products of an anomalous stomach digestion, and the result may, or must be, a necrosis in the form of a scab, first of the mucous membrane, and, when the disturbances to nutrition extend more deeply, of the other tissues of the wall of the stomach. In conclusion, we must not deny that it is possible that in particular cases a simple erosion, a catarrhal ulcer, or a diphtheritic infiltration may impart an impetus to the development of a round ulcer.

We see, therefore, that the loss of substance, which we designate as ulcer of the stomach, may be the result of totally diverse pathological processes in the walls of the stomach; there is, consequently, as Engel long ago asserted, nothing specific about it.

As regards the disposition to the development of ulcer of the stomach, if we, in the first place, consider the question of sex, it may be regarded as certain that the affection is at least twice as prevalent among women as among men. All statistical accounts agree on this point. As regards Erlangen, the journals of our pathologico-anatomical institution yield the following proportions. Of fifty-three subjects in whom ulcers or cicatrices were discovered in the stomach, thirty-eight (71·7 per cent.) were of the female, and fifteen (28·3 per cent.) of the male sex.

With regard to the influence of age there is not the same concordance of testimony. The general opinion that the disease is most prone to occur in adolescence and middle age, and, on the other hand, that the very old and the very young are least liable to it, is apparently disproved by Brinton's statistics, according to which the frequency of the affection constantly increases from ten years up to the most advanced old age. The proof of these estimates is, however, only apparent, there being, in my opinion, an erroneous basis for the conclusion. Brinton, that is to say, depends upon the fatal cases only, and has apparently taken into account only those numbers which represent the ages of the individuals at the time of their death. The matter in question for us is the determination of those periods of life in which the ulcer of the stomach first becomes developed, and not of those in which it has a fatal issue, or is accidentally found after death. Clinical experience teaches us that only a small fraction of patients suffering from ulcer of the stomach succumb to the first attack, that the majority attain to middle or even advanced age, and that they die either from relapses, or from the effects of the ulcer, or from other diseases. We see that this question can only be answered by clinical experience, and this teaches us positively, that the disease exhibits a preference for adolescence and middle age.

Among the causes of the connection of this disease with adolescence we must place in the first rank the alterations in the composition of the blood and the derangements of nutrition, which are connected with the development of the body during and after puberty. Chlorosis and anæmia must especially be mentioned. The frequency of the coincidence of these deranged conditions of the blood with ulcers of the stomach admits, *à priori*, of the conclusion that there is a causal connection between them; and, as a matter of fact, we obtain some notion of this from the demonstrations of Virchow and Rokitansky, who found a peculiar thinness of the walls, and a narrowing of the calibre of the vessels, as well as the frequent occurrence of premature fatty degeneration in the coats of the blood-vessels, in a girl suffering from chlorosis. This condition of the blood-vessels allows us to assume an increased friability of the walls of the smallest arteries and capillaries, and renders it probable that in such patients a relatively trivial amount of mischief may so act as to lead to a circumscribed hæmorrhagic infiltration.

Ulcer of the stomach is likewise frequently associated with tuberculosis and chronic pneumonia. A definite causal connection cannot yet be proved, and the question must for the present remain undecided, whether this combination is only an accidental one, dependent upon the absolute frequency of both processes, as Bamberger thinks, or whether, on the contrary, the weakness of the constitution, and the multifarious derangements in the nutrition of the tissues, consequently also of the walls of vessels, which we find occurring at an early period in individuals hereditarily predisposed to phthisis—I refer to the tendency to epistaxis and hæmoptysis—may serve as points of departure for both processes at the same time. It is possible that the derangements of assimilation and general nutrition, which attend ulcer of the stomach, accelerate the development of the lung affection; at least, we often notice that the latter is preceded for many years by the symptoms of very severe ulceration of the stomach.

After what I have laid before you with reference to the origination of the ulcers in embolism and thrombosis of the smallest branches of the gastric arteries you will understand how it is that affections of the endocardium and of the internal tunic of the vessels occur pretty frequently in combination with ulcer of the stomach. With reference, finally, to the question, as to how far derangements of innervation, upon which Siebert has laid especial stress, have any influence in disturbing the circulation in the coats of the stomach, we cannot at present decide; at any rate, however, considering the importance, daily more and more clearly demonstrated, of the nerves of the vessels and their affections with regard to the conditions of the circulation and nutrition of the organs, the existence of such an influence cannot be directly denied.

All those injurious influences to which the stomach is daily exposed may, as a matter of course, act as occasional causes;

and mention may be made particularly of such as excite or maintain hyperæmic and inflammatory conditions in the gastric mucous membrane. Among these are hard, indigestible articles of food, liquids either too hot or too cold, containing much alcohol, or otherwise stimulant. The frequency with which ulcer of the stomach occurs among servants, and especially cooks, a fact upon which Bamberger lays stress, and which my own experience completely confirms, may, in part at least, be thus explained. I venture to think also, that a traumatic origination of the circumscribed hæmorrhage is not of very unfrequent occurrence. Blows upon the gastric region, compression of the stomach, as occurs in persons leading sedentary lives, or in women from tight lacing of their dress, or in men from tight girdles, especially when digestion is going on, the pressure to which the stomach is exposed from the action of the abdominal muscles in vomiting, or in painful efforts at defæcation,—all these mechanical influences, together with the delicate condition of the walls of the vessels, and especially when conjoined with the hyperæmia consequent on digestion or possible catarrhal excitement, are well calculated to produce rupture of the small vessels, and consequent hæmorrhagic infiltrations.

A few words, in conclusion, with reference to the geographical distribution of ulcer of the stomach. This may justly be said to be very unequal; the statistics of Chambers, Gairdner, and others, with regard to England, show that open ulcers or cicatrices occur in from 2 to 3·5 per cent. of all dead subjects; in Prague, according to Jaksch, the proportion was about 5 per cent.; in Berlin, according to the statistical compilation from Virchow's pathological institute, 4·2; in Jena, according to Starke, with regard to Gerhardt's clinic and polyclinic, 10 per cent.; and in Denmark, according to Dahlerup, even 13 per cent. was reached. Dahlerup's statements rest, however, upon too slight a basis of statistics for them to be considered as possessing a general value; but, so far, I can confirm the frequent occurrence of ulcer of the stomach on the shores of the Baltic, for in my former sphere of labour at Greifswald I found the proportion to be very high. In Erlangen it is considerably less. Of 1,166 dead bodies examined in our pathological institute from 1862 to 1869, it was only in 53 that ulcers, or their remains, were found. Here we have a proportion of 4·55 per cent., which accords almost completely with the Berlin statistical estimates.

The anatomical changes may be very briefly described. The primary stages of the development have not as yet been sufficiently studied. After the nutritional disturbances have set in, the scabbing and the softening of the hæmorrhagically infiltrated portions appear to go on with great rapidity. We then find a sharply-defined, round spot of mucous membrane converted into a blackish, pulpy, or drier scab, and after this has separated, the submucous connective tissue, or the muscular coat, becomes exposed, or, if both of these are destroyed, the serous covering only may remain. Cases of burns involving a large extent of the skin, afford us the most frequent opportunities for observing this early stage; it may also be seen when a wound or an embolus is the cause of the ulcerative process. The incipient stage of loss of substance in two places, evidently produced by emboli, was found by Merkel in the wife of a prebendary, 94 years of age. The mucous covering in part remained, but changed into a mass of black shreds, still firmly adherent to the subjacent parts. In like manner here, recently, in a post-mortem examination of a woman 67 years of age, we found in the stomach five ulcers, lying almost in a straight line, one after the other, and probably of thrombotic origin. Of these, three were left smooth and clean, while the other two were covered by felt-like, shrivelled remains of the mucous membrane.

On the other hand, in the case of a man who, in consequence of a strangulated hernia, had vomited violently for several days, the matters ejected being at last streaked with blood, Rindfleisch found, in addition to several smaller hæmorrhagic infarctions, two circular patches at the small curvature, one of which presented itself as a hæmorrhagic infarction of the gastric mucous membrane, while the other exhibited all the characters of the pure, simple ulcer.

This case of Rindfleisch's, as well as the rarity of reports of that kind, renders it probable that the necrosis and the separation of the necrotic portions go on very rapidly under the influence of the gastric juice. The loss of substance, appearing after separation of the scab, is of longer duration, and therefore of more frequent occurrence in anatomical reports. The extension of the primary defect laterally and deeply depends, apparently, in an especial manner, upon the extension of the circulatory derangements, that is, upon the size of the blood-vessel which has become impervious, or upon the extent of the hæmorrhagic infiltration; but a renewal of the defect in the deep structures may always be determined by the presence of deeply burrowing and, particularly, perforating ulcers, funnel-shaped, with terrace-like intervals.

The ulterior changes, the gradual extension of the loss of substance laterally and deeply, must, for the most part, be attributed to the peptic operation of the gastric juice. This deleterious influence of the gastric juice, or of the corrosive products of the acid fermentation of the contents of the stomach, particularly of the acetic and butyric acids, is explained in part by the want of the protective epithelial covering, in part by the insufficient supply of the alkaline blood to the base of the ulcer. The neutralisation, by the alkalies of the blood, of the acid which has penetrated the tissues, has now, as was first insisted on by Virchow, become insufficient, in consequence of the disturbances to the circulation. Whether the ulcer rapidly or slowly spreads, whether it perforates or becomes spontaneously circumscribed, and goes on to granulation and cicatrisation, depends upon the condition of the blood-vessels in the base of the ulcer, and upon the nature of the contents of the stomach.

It is evident that the blood-vessels must become involved as the corrosion of the tissues advances. But if, in spite of this, bleeding from the stomach, especially to a marked amount, is of proportionately rare occurrence, that is, is found only in perhaps one-third or one-fourth of the cases, the reason for this may well be, on the one hand, that the arrosion of the smaller vessels is preceded by a thrombosis of their lumen, and, on the other, that the larger vessels, from their anatomical position, fall within the range of the destructive process only when the deepest parts are invaded. But even when the arrosion attacks a vessel which has not become the seat of a thrombosis, the formation of a thrombus may occur in spite of considerable loss of blood, and this prevents, at least for some little time to come, any danger of death by hæmorrhage. This, however, can only be assumed with regard to the smaller vessels, particularly the veins. If the arrosion attacks somewhat important arteries, the hæmorrhage must necessarily lead to a fatal result, and this is observed most frequently in arrosion of the gastro-duodenales, coronariæ ventriculi, and gastro-epiploica dextra arteries, in accordance with the usual localisation of the ulcers in the small curvature and the pyloric portion. I myself have had the opportunity of observing an almost instantaneously fatal hæmorrhage, produced by an arrosion of the splenic artery.

Our experience teaches us that the slowly spreading ulcers, notwithstanding the often extensive destruction of the coats of the stomach, threaten life in a less degree than those running a more acute and, at the same time, latent course. These latter, particularly when situated on the anterior wall of the organ, often penetrate the coats of the stomach in their entire thickness. In those more slowly extending ulcers of the small curvature and its neighbourhood, a barrier is opposed to the progress of the destruction by the adhesion of the serous covering to adjoining organs, especially to the pancreas and the left lobe of the liver.

It is true that when the ulcers have completely healed, these close adhesions of the stomach to the pancreas and the left lobe of the liver, &c., not seldom cause permanent interference with the freedom of movement and peristaltic action of the stomach, and, combined with the contraction of the cicatrices in the wall of the organ, cause distortions and alterations of shape, which not only add to the difficulties of the movements in digestion, but are probably also frequently the causes of those violent cardialgias which so often continue even after the ulcer has been completely healed.

There is no doubt that, in spite of close adhesions, an improvement, or even a spontaneous cure, may occur in the course of years, provided that the base of the ulcer is formed by the wall of the stomach, and not by any neighbouring organ. The original adhesion is in time gradually drawn out by the peristaltic movements of the stomach, and is made to assume a rope-like form, and this, again, in consequence of the persistent strain to which it is subjected, becomes thinner and thinner, and at last snaps. The reports of cicatrices and thickened spots on the serous covering, without adhesions to neighbouring parts, and also the frequency of analogous processes in the pleura and pericardium after partial adhesive pleuritis and pericarditis, may well support us in assuming that the hindrances to the movements of the organ may be thus naturally remedied.

Much more serious, however, are the consequences of cicatrisation of large circular ulcers at the orifices or the small curvature. In consequence of the progressive contraction of the cicatrices, we have strictures of the orifices, particularly at the pylorus, or hour-glass constrictions of the entire organ, and these interfere to a very great degree with the functions of the stomach. First and foremost in this respect is stricture of the pylorus from a cicatrix with secondary gastrectasis, which is often the object of treatment many years after the ulcer has been completely healed.

The diagnosis of ulcer of the stomach is in many cases unattended with difficulty; the coincidence of the more important symptoms, especially the cardialgia, the derangements of the gastric digestion, vomiting, bleeding from the stomach —the fact of the difficulties continuing for years, the nutrition of the body being relatively good, and the disease appearing to pause from time to time—these, added to the fact that the patient is of young or middle age, render it possible for our diagnosis to be a certain one. Every experienced physician knows, however, that often, in the form of the disease with which he has to deal, one or several of these more important symptoms may be absent. Especially in patients in middle life it may be impossible to diagnose between ulcer of the stomach, cancer of the stomach, and chronic gastric catarrh, in cases where the malady has not been of long continuance, where there has been no bleeding, and but slight pain in the stomach, where the physical examination renders us no assistance, and where the nutrition generally has become affected. Not less difficult is the diagnosis in young people suffering from chlorosis and anæmia, if the derangements of gastric digestion have not been of long continuance, if there is but little pain in the stomach, and slight sensitiveness of the epigastrium, and if accompanied by neuralgic troubles in other regions, and if there is an absence of such symptoms as bleeding from the stomach, increase of pain, and vomiting after eating. It frequently happens that as the disease progresses the true state of the case is made evident by the occurrence of bleeding from the stomach, or perforation. On the other hand, when the issue is favourable the diagnosis remains doubtful, even up to the termination of the treatment, and perhaps only when a relapse occurs, with sharply defined symptoms, are we able to come to a conclusion as to the nature of the previous malady.

These difficulties in the differential diagnosis of gastric ulcer, the frequently paradoxical nature of its symptoms, but particularly the difficulties in distinguishing ulcer from the nervous cardialgia of chlorotic girls, or young women suffering from uterine affections, and, on the other hand, from chronic gastric catarrh, and from cancer of the stomach in advanced and middle life, make it appear desirable to be able, in doubtful cases, to make use of the success or non-success of therapeutical efforts as data on which to base a conclusion with regard to the nature of the affection. In fact, the experiment has been repeatedly made of establishing the diagnosis according to the effects of nocentia or non-nocentia, or juvantia. For this purpose very indigestible articles of food, or arsenical solution (according to Siebert's proposal), have been introduced into the stomach, and conclusions have been drawn as to the presence or absence of ulcer from the immediate occurrence of exacerbation, or improvement, or absence of change in the local symptoms. Experiments of this kind are, however, not only hazardous, but unreliable as regards results. On the other hand, according to my experience, the result of careful and continuous treatment by alkalies, in a way presently to be described, may, with due precaution, be taken advantage of in making a diagnosis from the effects of juvantia and non-juvantia, inasmuch as by far the greatest number of ulcers of the stomach, and also of chronic gastric catarrh, are cured, or permanently benefited, by this treatment; whereas, on the other hand, it only transiently, and for a few weeks, alleviates the symptoms of cancer of the stomach, and either is without effect on the nervous cardialgias, or directly aggravates them.

I now turn to the most important portion of my task, viz. the treatment of ulcer of the stomach.

Since Cruveilhier, all authors have agreed in laying especial stress on the importance of the regulation of the diet; indeed, many have gone so far as to assert that dietetic treatment alone could be attended with curative results, and that the only effect to be expected from medicines was a palliative one, such as relieving pain and neutralising acidity. At a later period, on the one hand, a certain amount of confidence has been placed in the nitrate of bismuth and the nitrate of silver as effective remedial agents, while other astringents, as acetate of lead, alum, tannin, and chloride of iron, have found a few adherents. Besides these, during the last ten years, the curative effects of the Karlsbad springs have been much extolled; and this not only by the Karlsbad physicians, but by truly unprejudiced authors, such as Jaksch, Oppolzer, and von Niemeyer. There is, lastly, a pretty general concordance of opinion, that we can scarcely dispense with narcotics to subdue the cardialgia, with cold to check bleeding from the stomach, or with carbonate of soda to restrain the enormous production of acid in the stomach. This is the present state of our knowledge with reference to the therapeutics of ulcer of the stomach.

In order that we may comprehend, as precisely as possible, the indications for the treatment of ulcer of the stomach, the causal conditions must be regarded as the ultimate objects of inquiry. To what conditions does ulcer of the stomach owe its origin? What causes affect the spread of the ulcer, and the dangers therewith connected? And what, in conclusion, are the obstacles to a spontaneous cure?

As regards the first question, I have already briefly alluded to what, in the present state of our knowledge, must be regarded as predisposing and occasional causes. If we are able, at an early period, to remove these causes, we shall, as a consequence, often prevent the development of ulcer of the stomach. Careful and early treatment of chlorosis and anæmia, judicious regulations for the avoidance of those pernicious agencies which may cause hæmorrhagic infiltration of the gastric mucous membrane; above all, careful regulation of the dietetic conditions, and the improvement of the general nutrition—these are, perhaps, the main points in a rational prophylaxis of ulcer of the stomach. These statements may at the first glance appear to be somewhat theoretical, but their great utility is proved in practice, particularly in individuals who have already suffered once or several times from pronounced ulcer of the stomach, and who, therefore, may be certainly regarded as especially predisposed thereto. I shall return once more to this subject when I come to describe the after treatment.

The answers to the second and third questions which I have just now put before you, that is to say, what agencies may be considered as causes of the extension of the original defect, and what, as obstacles to its cure, are naturally of much greater practical importance; for, in proportion to the number of cases, an opportunity is but seldom afforded us of adopting prophylactic measures. It much more frequently happens that the ulcer comes under treatment in an advanced stage, after it has existed for a long time, and has been neglected and badly treated. Let us endeavour to give an answer to these two questions.

We have seen that the extension of the loss of substance due to the original hæmorrhagic necrosis, may be principally, if not entirely, attributed to the corrosive influence of the gastric juice, and that this is the most essential obstacle to the formation of granulations in the floor of the ulcer, and, consequently, to the healing process. We are, therefore, necessarily led to the conclusion that the neutralisation of the acids of the stomach is the first and most important task in our therapeutical efforts. If, when the healthy stomach is poisoned by mineral acids, we introduce, as quickly as possible, substances to neutralise these, and to prevent the deeper spread of the corrosive effect, we can, in my opinion, have no scruple in making use of the same treatment, it may be of a less violent, but of a more consistent and protracted character, in the case of ulcers of the stomach. In whatever stage the ulcer may come under treatment, whether quite recent or of long standing, the urgency of the indication is in nowise affected; for it will always be our first object to guard against the further extension of the digestive necrosis towards the serous covering, and to protect the young granulations from the dissolving influence of the gastric acids and the pepsine.

A simple watery solution of carbonate of soda suffices for the thorough neutralisation of the acid of the gastric juice, provided that the alkali be continuously administered. The solution should be only slightly concentrated, and a large quantity of it should be taken when the stomach is nearly or quite free from food. With regard to this, Gerhardt has expressed some scruples, which from his stand-point are thoroughly justifiable. "The ulcer of the stomach," says Gerhardt, "would be placed under the most favourable conditions for the healing process if the contents of the stomach were rendered neutral by the constant introduction of alkalies. This, however, is impracticable, for it would render impossible the assimilation of albuminous nutritive material, and thus most seriously interfere with the nutrition of the patient." However correct the conclusion may be, I cannot agree with Gerhardt. According to my observations, a permanent neutral condition of the contents of the stomach is certainly not necessary; it is sufficient that this neutralisation should be made to occur once a day, but always upon the condition that at the same time, at least once a day, the contents of the stomach should be emptied into the intestine. This regular, daily, and complete removal of the acid chyme from the stomach is indispensably necessary. For the attainment of this object the carbonates of the alkalies, however, are not sufficient. For this purpose we require rather an alkali which excites strong peristaltic movements of the stomach, but which is not followed by unpleasant irritation of the gastric mucous membrane, and especially of the structures on the floor of the ulcer. Such properties are possessed in a marked manner by the sulphate of soda. Glauber's salt is, according to my experience, the alkali which must be regarded as the most important in the therapeutical treatment of ulcer of the stomach, not only because it acts promptly and certainly in evacuating the contents of the stomach into the intestines, but because it at the same time positively checks or prevents the acid fermentation of those contents. In both respects the effect of common salt is similar to that of Glauber's salt, since it also has the property of restraining the fermentation and putrefaction, and, at the same time, a gently stimulating effect upon the muscular coat of the stomach and intestines. The latter effect is, however, much weaker than that produced by the Glauber's salt, and in the majority of individuals quite insufficient.

These three substances, the Glauber's salt, carbonate of soda, and common salt, form, as is well known, the most important constituents of the Karlsbad waters. To these, in combination with the high temperature and the carbonic acid, is due the universally recognised marked efficacy of these springs in chronic affections of the stomach and bowels. The remaining fixed constituents—the carbonate of lime, the sulphate of magnesia, the sulphate of potash, the carbonate of the protoxide of iron, and the silica—have no influence upon the sanative effect, and are, at all events, not requisite. Great importance, also, is not to be attributed to the free carbonic acid, for, according to my observation, it is not absolutely necessary for the remedial effect.

The efficacy of the Karlsbad springs in chronic ulcer of the stomach has been recognised for more than twenty years. The recommendations of the most able of the Karlsbad physicians, as Seegen, Hlawaczek, and Fleckles, are confirmed by Jaksch, Oppolzer, von Niemeyer, and others. If for all that, in every-day practice, the usual custom prevails of attempting to cure ulcer of the stomach by astringents, such as the nitrate of silver or the nitrate of bismuth, these remedies being, in my opinion, far inferior to the alkalies, the reason for this is that the course of treatment at Karlsbad is restricted by an old custom to the summer time, and that the high price of the water, when conveyed to a distance, makes it inaccessible to all but the well-to-do classes. I would in the most impressive manner counsel you not to follow too strictly the prevailing tradition of prescribing the water cure only in summer, and of exhibiting the alkalies only in the form of natural mineral waters. You can, with the same good results, prescribe the Karlsbad waters to be taken in winter, and even by patients permanently bed-ridden. You may also make use of the sulphates and carbonates of the alkalies in combination, as they occur in the so-called natural and artificial Karlsbad salts, or mixed in other proportions and dissolved in simple warm water. The effects will be found to be equal to, and in many cases, as I shall hereafter explain, even greater than those of the Karlsbad water.

With reference to the treatment of ulcer of the stomach, the three above-named alkalies fulfil the three primary indications already briefly pointed out, provided that the dietetic regimen, hereafter to be noticed, is also adopted. These alkalies neutralise the normal or abnormal acids of the stomach; they prevent for the time the acid fermentation of the contents of the organ; they provide, lastly, for a daily regular emptying into the intestine of all that the stomach contains.

Each of these three points deserves an equal amount of consideration. When the ulcer is commencing, the neutralisation of the contents of the stomach is above all things necessary, in order to check the progress of the destruction. Afterwards, when the ulcers have become stationary, it rather becomes necessary to guard against the fermentation of the contents of the stomach, and ensure the timely emptying of the stomach as the most important preliminary condition for the formation of granulations in the floor of the ulcer. It is the permanently intensely acid condition of the contents of the stomach which I regard as especially detrimental; this, however, is not a consequence of the ulcer of the stomach, but rather of its constant companion, the chronic gastric catarrh. Experience teaches us that as long as this catarrh is absent, the difficulties of the patient are inconsiderable, and are confined to attacks of cardialgia, or to a feeling of pressure and fulness after eating, or a sensitiveness to tight pressure of parts of the clothes; the troublesome pains occur only with gastric catarrh and its consequences. The decrease of appetite, the dyspeptic troubles, the belching and vomiting, the pyrosis, the permanent sensitiveness and swelling in the region of the stomach, the constipation, and, lastly, the emaciation and the general anæmia—all these are in most cases only the consequences of chronic gastric catarrh. The gastric mucus produced in abnormal quantity acts as a ferment, and causes acid fermentation among the ingesta, and the greater the quantity of the carbo-hydrates, which are most relished in consequence of the aversion to meat and other protein compounds, the greater the disposition of these to fermentation (as in the case of beer, and other fermented liquors), and the longer the ingesta remain in the stomach, the more prone is the fermentation to occur.

The point last alluded to—the obstinacy with which the chyme remains in the stomach—deserves very particular attention. That this very often occurs can be easily proved. If we frequently examine the matters vomited, the more or less altered remains of victuals taken thirty-six or forty-eight hours previously may be demonstrated in the highly mucous, sour-smelling liquid. It follows from this that the food is not passed on into the intestine at the proper time; indeed, the liquids are not so much as carried away or re-absorbed by the coats of the stomach; on the contrary, they are retained in the stomach until, by vomiting, the organ is relieved of their troublesome presence. In such cases it happens that, although the entire contents are not evacuated, but only a large portion of them, the quantity of the matters vomited is nevertheless, as a general rule, astonishingly great.

In explaining this symptom great difficulties present themselves. To begin with, it is highly probable that the delay in the emptying into the duodenum of the contents of the stomach, and the regular, copious vomitings, are caused by mechanical obstacles in the pylorus, such as temporary closure or stenosis of that orifice; and yet the post-mortem examination may reveal the fact that the pylorus was sufficiently permeable. In other cases the further course, the frequent changes of the symptoms, the temporary complete disappearance of all the indications of stricture of the pylorus, and especially, in the last place, the recovery of the patient, are so many proofs that we can have had to deal only with a transient obstruction in the pyloric passage. We are therefore forced to assume that the considerable catarrhal swelling of the mucous membrane of the pylorus, in connection with a spasmodic contraction of the pyloric sphincter, excited and maintained by the irritation of the floor of the ulcer, may be the cause of a mechanical obstruction. We must further assume, that when the ulcer and the chronic catarrh have continued for some time, the energy of the muscular coat of the stomach becomes so diminished that the contractions are no longer sufficient to open the closely-shut sphincter. This assumption is supported, on the one hand, by the observation, that under such conditions a dilatation of the stomach becomes pretty rapidly developed, and on the other, by the demonstration of anatomical changes in the muscular coats—such as have been found lately by Merkel, in cases where the ulcer of the stomach has continued for a longer period without stricture of the pylorus, in the form of simple fatty degeneration, and in that of colloid and fatty degeneration by Kussmaul and Maier in cases of gastrectasis, as a consequence of stricture produced by cicatrices in the pylorus. It is supported, in the last place, by the closely analogous case of the urinary bladder, in which we may observe a permanent closure of the sphincter in inflammatory irritation of the neck of the bladder, and also when the energy of the detrusor muscle is diminished in advanced life.

There is, in conclusion, one more circumstance to be noticed, which explains the obstinacy of the acid fermentation, and consequently the permanence of a corrosive action of the contents of the stomach upon the floor of the ulcer. Kussmaul, by means of the stomach-pump, has recently demonstrated that as soon as a certain degree of paresis of the muscular tissue, and of dilatation of the organ has been attained, the acid fermenting contents of the stomach are never completely discharged, notwithstanding the frequent and copious vomitings. This remnant of the fermenting chyme remaining behind, induces the same change in all fermentable substances as soon as they are introduced. The fermentation process, once set up in the contents of the stomach, is thus continually maintained by unsuitable food, the gastric catarrh becomes permanent, and the floor of the ulcer is constantly exposed to the corroding action of the caustic acid chyme. The majority of sufferers from ulcer of the stomach are often in this condition for a long period before seeking medical advice. Notwithstanding the intense pyrosis, in spite of the acute pain in the stomach, partly from ignorance, partly from want of self-control, they go on eating and drinking their usual improper food and luxuries. As a source of evil, the most prominent position may be emphatically assigned, in Bavaria, to the beer, the so-called national beverage. Evening after evening sundry pints of this readily fermenting liquid are introduced into the diseased stomach. Towards morning the patient is relieved by the vomiting up of an intensely sour, grayish mucous liquid, which sets the teeth on edge. The taking of food is followed by pain in the stomach and pyrosis. But in spite of all this, the old habits prevail when evening comes round, and thus the acid fermentation never comes to an end in the stomach. No wonder that in this way the chronic gastric catarrh assumes a dreadfully inveterate character.

This description of the morbid processes renders intelligible the favourable effect of a judicious dietary; it explains at the same time the marked curative action of the above-mentioned salts. The carbonate of soda, the chloride of sodium, and the Glauber's salts neutralise the acids, and check the fermentation. They therefore suppress the corrosive action upon the floor of the ulcer, and the reflex contractions of the pyloric muscular fibres, and by their strongly excitant action upon the peristaltic movements of the stomach, they cause the fermenting liquids to be rapidly discharged into the bowel. It is evident that if the stomach is thoroughly emptied at least once a day, its contents are much sooner rendered temporarily alkaline, or neutral, and their tendency to fermentation restricted. The dilatation of the stomach—the consequence of those accumulations—becomes less as the course of treatment is persevered with, and the chronic gastric catarrh, which is kept up mainly by the retention of the ingesta, and their great acidity, gradually disappears.

As I have previously mentioned, we have at our disposal, when using the soda salts for curative purposes, on the one hand the natural or artificially prepared Karlsbad waters, on the other the solution of the Karlsbad Sprudel salts, the artificial Karlsbad salts, or other saline combinations. As a general rule, the employment of the natural or artificial Karlsbad salts, in a moderately concentrated watery solution, is preferred by me to that of the Karlsbad water itself. My reasons for this preference are as follows:—Many patients suffering from ulcer find that the water disagrees with them, as the saying is, whether they drink the Muhlbrunnen, the Schlossbrunnen, or the Sprudel at Karlsbad itself, or take, at their own home, imported water, or water artificially prepared, and after it has been sufficiently warmed. They suffer, that is to say, from lowness of spirits, from palpitation of the heart and congestion of the head, from fulness in the region of the stomach, from belching, from oppressed breathing, and flatulence; the appetite fails, the stools are scanty, and often for several days entirely absent. This condition of things not unfrequently continues during the whole course of the treatment. It is only removed by active purgation, that is, by a regular and thorough evacuation of the ingesta. The confined state of the bowels is the primary cause of those troubles, and the most important obstacle to the satisfactory progress of the case. This fact is well known to the Karlsbad physicians. Seegen states emphatically, that even if we commonly notice that the use of the Karlsbad waters gives a moderate impulse to the evacuation of the bowels, and causes the stools to become soft, yet that often an obstinate constipation occurs, which cannot be subdued by taking large quantities of water, but that other measures are necessary. It is well known that, in order to aid the evacuation of the bowels, it is the usual practice at Karlsbad to add to the last cups of water taken, as much Sprudel salt as the circumstances of the case may require. Frequently, however, this provision is neglected, or insufficiently attended to, and for this the patients themselves are, for the most part, to blame. Many either never seek the advice of a physician resident at the baths, or only do so at the commencement of the course, and when constipation occurs, guided either by their own ideas or the advice of their fellow-patients, they endeavour to remove it by increasing the quantity of water swallowed, and they are generally unsuccessful. Often, indeed, the physicians are prevented by great pressure of work from giving the necessary attention to each individual patient. Thus it happens that many patients return from Karlsbad unsatisfied and uncured. As a matter of course, it usually happens that errors in diet and other injudicious doings play an important part in this respect. In all cases my advice is, that you should supply patients going to Karlsbad not only with definite directions to seek the advice of a local physician immediately on arrival, and regularly afterwards, but that you should also make them thoroughly alive to the necessity of daily repeated evacuations of the bowels.

The troubles which have been mentioned, and which occur with less frequency when Karlsbad water, either from the springs or artificially prepared, is used at home, than when the patient visits Karlsbad, may be avoided by a systematic use of the Sprudel salts dissolved in hot water, in a degree of concentration corresponding to the requirements of each particular case. We have, then, certainly no Karlsbad water before us, but an impure solution of Glauber's salts, for the salt is composed, as I shall by-and-by more fully explain, almost entirely of sulphate of soda, with a little chloride of sodium and carbonate of soda. The eccoprotic effect is, however, more under control, and experience places beyond doubt—and this is the main point of all—the singular efficacy of such warm solutions of artificial or natural Karlsbad salts.

I have obtained the best curative results in those patients who have submitted themselves to a course of treatment extending over four or five weeks in my clinical wards. The careful superintendence of the evacuation of the bowels, the rigid diet, the bodily rest, the abstinence from mental work and mental emotions, and other injurious influences, can nowhere be so completely provided for as in a medical establishment.

I make my patients take every morning, fasting, a solution of from eight to sixteen grammes, or two to four drachms (one to two teaspoonfuls heaped up) of the salt to a pound (one pint) of water, which has been poured on when boiling, and then the whole cooled down to about 44° R. A quarter of a pint is taken every ten minutes. Two to three motions are necessary; if the bowels are moved only once, or not at all, an enema should be used, and on the following morning the quantity of the salt should be increased by one-half, or doubled, the quantity of water remaining the same. Subsequently, as a general rule, less concentrated solutions are sufficient, and then the patient may return to a teaspoonful of the salt to a pint of water. In cases where the gastric catarrh is very intense, and the pyrosis particularly obstinate, it will be found advantageous to administer, every evening during the first week, another bottle of an acidulous soda water (Giesshübel, Bilin, or Vichy).

Various mistaken views prevail with regard to the composition of the Karlsbad Sprudel salt and its artificial imitations. On the one hand, it is assumed by several that the real Sprudel salt contains the whole, or, at all events, the greatest part of the fixed constituents; on the other hand, the preparations met with in commerce under the name of artificial Karlsbad salts are regarded as being, as a matter of course, chemically identical with the real Sprudel salt. Both suppositions are incorrect. An analysis by von Gorup-Besanez shows that the artificial Karlsbad salt, prepared according to the usual formula in Hager's Manual, is almost entirely composed of Glauber's salt, and contains less than 1 per cent. of chloride of sodium and carbonate of soda, whereas the true Sprudel salt is also composed chiefly of sulphate of soda (87·14 per cent.), but contains in addition almost 13 per cent. of carbonate of soda, and only traces of chloride of sodium.

We see, therefore, that the Sprudel salt for the most part consists of Glauber's salt, and that the artificial preparation is almost entirely so composed, that the quantity of carbonate of soda is noticeable only in the real salt, and that only a minute quantity of chloride of sodium is contained in both. It follows from this that a solution of two drachms to the pound yields a much stronger water (that is, richer in Glauber's salt, but containing less carbonate of soda and common salt) than that of the Karlsbad springs; and in the second place, that the true Sprudel salt is inferior to the artificial preparation as regards the purgative effect, inasmuch as it contains less Glauber's salt, and that, consequently, it is necessary to give a somewhat larger dose of it, in order to obtain equal eccoprotic effects. If, as may be well supposed, the composition of the salts, especially of the artificial one, is not always exactly the same, we must the more regard it as proved that we have to do with an impure Glauber's salt, and that the therapeutical effects are almost entirely to be regarded as those of Glauber's salt, seconded by two powerful factors, viz. the large quantity and the high temperature of the water used for solution. The great majority of my experiments on stomach ailments of all kinds (about 300 altogether) have been made with the artificial salt. This has one essential advantage over the real salt, which is of importance in the case of poor patients, and that is, that it is cheap. A pound of the real salt costs two thalers (nearly six shillings) and more, whereas a pound of the artificial salt may be bought for eight or ten silver groschen (ninepence to a shilling), or even for less at the druggist's. Inasmuch as a pound of the salt is sufficient for the patient for a four weeks' course, the therapeutical treatment may be carried on even in the case of the poorest. With regard to the taste, I will just remark that the solution of the real salt tastes better than that of the artificial one, and that this latter is less nauseous than the pure Glauber's salt.

The next most important task of our treatment is the regulation of the diet. All authors are agreed that a careful selection of alimentary substances is indispensably necessary for curing ulcer of the stomach. There is, indeed, no doubt that in many cases a cure of the ulcer is effected by means of a dietetic regimen empirically tested and consistently carried out by the patient without the aid of any medicines. Experience has sufficiently pointed out to us the articles of food having a favourable, and those having an unfavourable, effect in morbid processes of the stomach. An explanation of these their effects is not difficult, as regards the majority of the ingesta. The injurious influence of dense and coarse articles which, like the majority of vegetable substances—for example, the legumes, fruit, the cabbage tribe, brown bread—contain a great amount of stiff, undigestible vegetable fibre, is explained partly by the mechanical irritation of the mucous membrane, and partly by the length of time that these articles remain in the stomach. The mechanical irritation, which is induced by the long-continued circulation of these articles, affects not only the floor of the ulcer, but also the relatively healthy portion of the mucous membrane, whose glandular apparatus is excited to a more copious secretion of gastric juice and mucus. The evil consequences become more prominent the harder and more acrid the substances used, and the greater the development of the gastric catarrh. To these evils of a mechanical kind are added those of a chemical nature which may be introduced in the form of organic acids (vinegar), or formed in the stomach by the process of acid fermentation. The more intense the catarrh, the greater the consequent disposition of the ingesta to acid fermentation, the more prejudicial is the effect of those carbo-hydrates which tend especially to the acid fermentation. Among these I include the fats, whose fermentation engenders butyric and other fatty acids, the saccharine matters, pastry, and beverages containing much sugar—beer, especially—which rapidly develope lactic and acetic acid in the stomach; to these may be added fat meat, fat puddings, and so forth.

Experience teaches us that the protein compounds, containing but little fat, are far less prejudicial. Roast veal or chicken, justly praised by Krukenberg, cold, and eaten without sauce, raw ham, white bread, milk, light claret—all these are nutrient materials, some of which either offer great resistance to fermentation, or directly counteract it; others, such as milk, are too quickly transmitted from the stomach into the intestine to be subject to any considerable changes.

The peculiar suitability of a continuous milk diet in cases of ulcer of the stomach and chronic gastric catarrh is placed beyond all doubt by the experience of thousands of cases. We must suppose that the milk, as has been recently and correctly insisted upon by Gerhardt, first of all as a weakly alkaline liquid, by means of its alkalies takes up a portion of the hydrochloric acid of the gastric juice, and that the pepsine is claimed by its albumen. On the other hand, as it seems, the milk is too rapidly transmitted from the stomach into the intestine to admit of the higher degrees of the acid fermentation. It is shown, at least, by the observations of Busch and Kühne, that milk, introduced by itself, leaves the stomach with marvellous rapidity. In spite of this, however, the tendency to acid fermentation is in many patients so great that it is even necessary to add an alkali, such as the bicarbonate of soda, to the milk, in order to preserve it from too rapid fermentation.

It unfortunately happens that in particular patients we find an unconquerable idiosyncrasy against milk; in such cases an experiment with buttermilk, as recommended by Krukenberg, sometimes answers the purpose. In general, however, there are but very few patients who are unable to take the milk when their diet is in other respects strictly regulated, and alkalies are employed. I have often convinced myself that the so-called intolerance of the milk diet is dependent only upon the continuance of acid fermentation in the stomach, kept up by the too infrequent evacuation of the contents of the stomach into the intestine, and by the surreptitious introduction of readily fermentable articles of diet. Nothing, after all, is more detrimental to our therapeutical efforts in ulcer of the stomach, than the experiments in all directions, in the matter of eating and drinking, which patients are so apt to make, either of their own accord, or by the advice of others. The best plan is to write down for our patients a minute and strict bill of fare, beyond which they must not go, under any circumstances whatever. This bill of fare should be as simple as possible. All finesse and refinements of gastronomy must be inexorably excluded. Elegance of cuisine, the pride of many physicians in good practice in large cities, must here be laid aside.

The course of treatment is as follows: Every morning, at first from one-and-a-half to two teaspoonfuls, later on only one, heaped up, of Karlsbad salt are dissolved in a pint of boiling water, and allowed to cool down to 44·45° R.; of this the patient, fasting, is to take a half-glass, that is, the fourth part of a pint, every quarter of an hour, the first portion in bed, the second after dressing, the third and fourth in the open air, with moderate exercise. In winter the entire quantity can be taken in the room, and even in bed. The bowels should be opened once or twice while the drinking is going on, or after this is finished. Should the bowels be moved either more or less frequently than this, the quantity of the salt should be diminished or increased on the subsequent days. Half or three-quarters of an hour after the water has been taken, the patient should have a glass of milk, or coffee with milk, with white bread. At half-past ten a second breakfast, cold roast veal or chicken, with white bread or raw ham, a glass of claret. At one o'clock, dinner, soup of meat or milk, roast veal or chicken, without sauce, white bread, a tablespoonful of mashed potato prepared with milk, a glass of claret. At four o'clock, half a pint of milk, with white bread. At seven o'clock, milk soup, or a pint of milk, cold roast meat, with white bread. Between eight and nine o'clock, from half a bottle to a bottle of an acidulous soda water, if necessary. Bedtime to be nine o'clock.

If the tendency to acid fermentation is only slight, or completely disappears during the first week, a little butter or a lightly boiled egg may be allowed with the second breakfast and the evening meal. The puddings often allowed at Karlsbad, the dried and stewed fruit, both often taken in excess at Karlsbad, are a frequent cause of indigestion; in addition to these, I strictly forbid strawberries, beer, and chocolate. The daily quantity of milk may be increased if it is taken willingly and well borne. I have no experience of the milk bread recommended by Budd, but a somewhat similar preparation, namely, milk soup prepared with fine wheat and rice-meal or pollard, as is much in favour in North Germany, has been found by me to be very suitable.

As regards the regimen in other respects, all mental and bodily exertion is to be strictly forbidden, and all sources of mental excitement must be kept at a distance. The gastric region should be protected against sudden cooling by a layer of flannel or fur. The action of the skin is to be stimulated from time to time by tepid baths, the effect of which is more refreshing when they contain carbonic acid.

Among medicines, the narcotics, especially morphia, are indispensable only at the commencement, to subdue the violent paroxysms of cramp in the stomach; after eight days they are often unnecessary. I urgently advise that the morphia should be used sparingly, and only in small doses, perhaps one-twelfth of a grain, for this generally does its duty, and if repeated several times in the course of the day, is sufficient to subdue almost entirely the pain in the stomach. As the best method of application, I recommend the subcutaneous injection, or a solution of two grains of morphia in half an ounce of water of bitter almonds, ten to fifteen drops to be taken twice or three times a day.

If the alkalies are used in this regular manner, the diet rigidly controlled, and the morphia employed as symptoms may indicate, the troubles of digestion, particularly the pyrosis, and commonly also the cardialgia, disappear for the most part in the course of the first or second week. The appetite improves, the skin and the mucous membranes acquire a rosy hue, the patient becomes lively and full of hope. Even when the ulcers have continued for a long time, the healing process goes on pretty rapidly, and a course of treatment extending over four weeks is almost always sufficient for the cure. Even after the termination of the course it is necessary that the proper diet, with, perhaps, somewhat fewer restrictions, and the use of simple acidulous soda waters, should be continued for some weeks.

When this method of treatment is strictly carried out, bleedings from the stomach occur but very rarely. This is well explained by the fact that the corroding influence of the contents of the stomach upon the floor of the ulcer is reduced or destroyed The intestines become filled, and the natural convexity of the previously flattened abdomen is restored, in consequence of the regular transmission of the contents of the stomach into the bowels. Notwithstanding the watery evacuations each morning, the assimilation of nutrient material goes on in a most gratifying manner. This is best shown by the rapid increase in the weight of the body, which I could prove to have occurred in my patients by having them weighed once a week. To mention only one example, in a case recently under treatment in our clinical wards, in which the ulcer of the stomach had existed for more than two years, and the man had become very much reduced, in consequence of oft-recurring hæmorrhage and great difficulties of digestion, the patient's weight during the course of treatment increased by eight pounds in fourteen days.

Relapses are not unfrequent, even under this treatment. Sooner or later, after the ulcer has completely healed, a fact which we may justly infer from the disappearance of all the more important symptoms, we have the renewed occurrence, often apparently attributable to errors of diet, of difficulties of digestion, pressure and fulness in the epigastrium, cardialgia and vomiting, and the disease again becomes rapidly developed to a considerable extent, unless the necessary alteration in the diet, etc., is promptly carried out. Never have I seen the methodical treatment by the alkalies produce such a brilliant effect as in cases where a relapse has again come under treatment. If the patients are made aware of the importance of the earliest signs of a relapse, and follow out the advice given, to present themselves immediately and to recommence the course as before, we may count on a removal of all the troubles in two or three weeks. I think I may assume that with the recommencement of the treatment the process of corrosion stops, and that consequently the granulation process is enabled to set in. All patients, whose own experience has convinced them of the rapid effect of the alkalies upon ulcer of the stomach and its relapses, become zealous advocates of this method of treatment, and are for the future only too prone, of their own accord, to treat themselves and others in this fashion. Of a series of patients, who years previously had been under my treatment for confirmed ulcers of the stomach, and in whom the relapses had been overcome in good time, I know that all have subsequently remained free from any serious contingencies.

As regards the other medicines recommended for ulcer of the stomach, I have at all events some experience of the efficacy of the nitrate of bismuth and the nitrate of silver, which I employed extensively in my first years of practice. I have only rarely seen a permanent result from the nitrate of silver, whereas in many cases the bismuth has exerted a favourable influence, when not given in too small doses. Naturally the effect of the bismuth is more evident to the patients themselves, when, as is generally the case in practice, it is combined with small doses of morphia. With regard to this, however, my opinion entirely coincides with Gerhardt's, that the morphia, by removing the pain in the stomach, lulls, so to speak, the patients' self-control and care as to diet, and frequently makes them thoughtless in this respect. I cannot, therefore, recommend the regular use of morphia several times a day, but I advise it to be used only when the paroxysms of pain are very violent and the vomiting obstinately continues.

However favourable the action of the subnitrate of bismuth in individual cases, it has, on the other hand, in many cases displayed no permanent, I might say no radical, effect; indeed, in several cases it has quite disappointed my expectations.

It is possible that this want of success depended in part upon insufficient attention to the rules regarding the diet. It is, however, an old experience that the patients follow out much more carefully the dietetic directions laid down for them when they are subjected to a methodical course, especially with mineral waters, than when medicines are prescribed in the ordinary fashion, "a tablespoonful every two hours," "a powder three times a day," and so on.

If I, therefore, cannot dispute the efficacy of the abovementioned remedies, which has been proved by many trustworthy observers, my opinion, nevertheless, is, that as regards rapid, certain, and permanent effects, the preference must be given to the methodical administration of the mixed alkalies.

Among the less frequent incidents in the course of ulcer of the stomach claiming the assistance of the physician, hæmorrhage from the stomach, and perforation of its coats, especially demand a brief discussion with reference to their treatment.

Cold is the most simple, and at the same time the most certain, means of combating hæmorrhage. The application of a light bladder of ice to the epigastrium, and the frequent swallowing of small fragments of ice, together with uninterrupted rest in the recumbent posture, and continuous absolute abstinence from food and drinks, are measures sufficient to check the generally controllable hæmorrhage. Mental excitement, talking on the part of the patient, excessive heat of the room, anxiety and disturbing influences of all kinds, must be guarded against and forbidden. With regard to the astringents recommended in handbooks, such as alum, liq. ferri sesquichloridi, acetate of lead, and others, I should advise you not to make use of them—at least at the commencement. On the one hand, when the stomach is quite full of coagulated blood, these remedies can have no effect upon the central seat of the hæmorrhage; and on the other, they usually increase the nausea and the vomiting, and are thus very apt to cause a return of the hæmorrhage, which had already ceased. Every act of vomiting, every great distension or movement of the walls of the stomach, loosens the provisional thrombus in the narrowed lumen of the vessel. In hæmorrhage from blood-vessels of internal organs, which are not accessible to compression, we are, in a general way, compelled to adopt indirect rather than direct means of checking the bleeding. Ice, as I have already said, answers the purpose, if continually used internally and externally; internally, however, only in the form of small fragments, in order to prevent the stomach from becoming distended by larger quantities of water. As an indirect means of subduing hæmorrhage, I attach especial importance to the most perfect rest of the whole body, and of every separate muscle. Every strong muscular action increases the lateral pressure against the thrombus, already in jeopardy, and from this point of view we must regard as dangerous the mustard plaisters to the calves of the legs, a remedy in other respects so popular and harmless; and likewise clysters as derivatives, inasmuch as the adoption of either of these necessitates movements on the part of the patient. Officious meddling on the part of the physician is quite out of place.

On the second and third days after the hæmorrhage, alum whey, made cold with ice, is a suitable beverage; but it must only be given by mouthfuls at a time, in order to avoid filling the stomach, and the consequent danger of a recurrence of the hæmorrhage. To open the bowels, just one tepid enema may be administered; in no case are aperients, even of the mildest character, to be employed. I witnessed a relapse of the hæmorrhage on the fifth day in a case where a spoonful of castor-oil had been given.

The administration of nourishment may be re-commenced, with the greatest precautions, on the third or fourth day. The diet must at first be limited to milk and broth, with white bread, and perhaps a little iced champagne.

Perforation of the coats of the stomach requires the administration of opium in very large doses, principally with the view of insuring euthanasia, but also to tranquillise those movements of the stomach and bowels which counteract that which is aimed at, but which is almost always hopeless of achievement, viz. the sacculation of the perforation, and of the contents of the stomach extravasated into the sac of the peritoneum. As regards the relief of the pain, and restraining the movements of the bowels, opium acts best when administered subcutaneously or in the form of a suppository. The great tension of the abdominal walls, and the rapid setting in of peritonitis, are best combated by the application of a large bladder of ice, or if this should be too heavy, by an iced cataplasm. Among restorative remedies, I should recommend iced champagne as the best to be employed.

I wish, in conclusion, to draw your attention to the treatment of a few consecutive troubles which are not unfrequently a source of anxiety after the ulcer has been successfully cured. There are, on the one hand, the mechanical restriction of the peristaltic movements due to the adhesion of the stomach to neighbouring organs, and the stricture of the orifices caused by the gradual contraction of the substance of the cicatrices at the pyloric or cardiac openings; on the other hand, the atonic debility of digestion, the tendency to dyspepsia and pyrosis, and, finally, the habitual obstruction.

Of the above mentioned consequences, the stricture of the orifices due to cicatrices is by far the most serious. Since, as we have already seen, the neighbourhood of the pylorus is the favourite seat of the ulcers, it follows that the secondary strictures due to cicatrices of that orifice are those which are most commonly observed. The nearer the ulcer to the pylorus, and the larger the ulcerated surface, the more probable is the development of a gradually increasing contraction of the orifice. If the gastric catarrh still continues to some extent, or recurs, retention of the contents of the stomach, with acid fermentation, takes place; and this is accompanied by attacks of vomiting from time to time, which gradually become more frequent, and recur regularly after meals. If the bowels are obstinately confined, the lower part of the abdomen falls in; there are, lastly, physical signs of a dilatation of the stomach, and the existence of a cicatricial stricture of the pylorus can be scarcely a matter of doubt.

Less characteristic are the signs of distortion or constriction of the middle portion of the stomach; in this case also we have the troubles of digestion and assimilation, cardialgia, and attacks of vomiting.

In consequence of the rare occurrence of ulcers in its neighbourhood, the cardiac orifice is but seldom found affected with stricture caused by the cicatrices of simple ulcers. A very instructive case of the kind came before us in a post-mortem examination a short time ago, the patient having been for several years under observation and treatment in our clinical wards. The diagnosis could not be doubtful, for in a man 33 years of age the stricture of the cardiac orifice had existed for several years, preceded by the symptoms of ulcer of the stomach, and the mechanical dilatation of the stricture by means of sounds had been attended with very satisfactory results as regards the taking of nourishment and the general nutrition of the body. Death ensued from perforation of the stomach, due to profuse indulgence in new fermenting beer and new brown bread. The patient lived ten hours after the perforation had occurred. I regarded it as certain that a new ulcer had been the starting-point of the perforation; and the post-mortem examination convinced us that there were only old radiate cicatrices at the curvature, and cicatricial rings, causing stricture at the cardiac and pyloric orifices, and that a softening and a tearing of the fundus, as though it had been slit, was the cause of the perforation. The greater part of the acid contents of the stomach was found in the abdominal cavity, not, however, in the form of a circumscribed effusion in the neighbourhood of the fundus, with softening of the surrounding parts, as is wont to occur in post-mortem softening of the organ, but diffused throughout the whole abdominal cavity, covering all the coils of the intestines, even those situated in the sac of a hernia, with a uniform thin layer; a state of things which must have resulted from the movements of the intestines during life. The peritoneum was pretty uniformly and considerably injected. Moreover, the characteristic symptoms of perforation were made out with certainty shortly after it occurred, that is, eight hours before the death of the patient.

We had therefore here to deal with a softening and rupture of the fundus of the stomach originated during life, and due to the copious introduction of fermenting liquid and dense substances, and to the great development of acetic and carbonic acids, it being impossible to empty, either upwards or downwards, the enormously distended and tense stomach, in consequence of the strictures existing at the orifices.

The circumstances were, as you see, particularly favourable for the occurrence of softening and rupture of the fundus of the stomach during life. So complicated a condition may very seldom occur; but the case proves (what has been always doubted, notwithstanding the observations of Rokitansky, Hoffmann, and others) that gastromalacia may occur during life, and may cause death by rupture of the softened coat of the stomach.

The treatment of cicatricial strictures of the cardiac orifice can, as a matter of course, only consist in mechanically dilating the rings of the stricture by the daily introduction of œsophageal sounds, whose thickness must be gradually increased. In this manner, with the necessary amount of perseverance on both sides, as good effects can be obtained as in cicatricial stricture of the œsophagus following caustics or burns. In the patient just alluded to, as likewise in a second case which, unfortunately, was subsequently withdrawn from our observation, the result of daily treatment with sounds, beginning with very thin ones, and gradually increasing the size, was so far successful that the stricture was dilated to such a degree that meat, bread, and potatoes could once more pass through it. As a consequence, the state of the patient was soon changed from that of intense inanition to one which was entirely satisfactory. Exacerbations occurred from time to time, and the patient lived in our wards, employed in light work, as an assistant-attendant, for two years; and that he succumbed so early may be attributed to the fact that the sound was not used during the last year, and that for the few days before his death, he had taken scarcely anything but easily fermentable substances. We could certainly have saved the patient's life had we, on the critical day, been in possession of Kussmaul and Weiss's excellent stomach-pump, which would have enabled us to disburden the enormously distended stomach by pumping out the acrid and sour ingesta, and the gases.

The strictures of the pylorus caused by cicatrices are, unfortunately, inaccessible to such a method of treatment. We must here be content with prescribing a suitable diet, and opposing any existent catarrh of the stomach by the administration of alkaline waters, and any possible gastrectasis by the use of the stomach-pump. As regards the diet, there must be the same restrictions which I have recommended in the case of open ulcer of the stomach. Cold white meat, raw ham, milk, milk soup, meat broth, extract of meat, white bread, and claret, form the best dietary, for there is here the same indication to supply the easily digestible protein compounds which have no tendency to acid fermentation.

In order to prevent the accumulation and acid fermentation of the contents of the stomach, and the consecutive dilatation of the organ, it is necessary that it should be completely emptied every day, and that anything that may possibly remain behind should be neutralised. This is effected by a course of the Karlsbad salts extending over some weeks, or by the use of the stomach-pump, which may be used every day in the early morning. Kussmaul's favourable cases prove that the stricture of the pylorus may go on up to complete closure if the stomach becomes dilated and the muscular coat paretic, in consequence of the accumulation of the fermenting chyme; they show, on the other hand, that the permeability of the orifice may be improved by the repeated use of the stomach-pump, and by subsequently washing out the organ with soda water.

By the use of the stomach-pump Kussmaul obtained very favourable results simply by removing those consecutive conditions, and in spite of the persistence of the cicatricial stricture. Bartels obtained a like favourable result by prescribing a "dry diet," that is, meat and white bread, with very small quantities of fluid, which only very partially fill the stomach and resist the acid fermentation. (I can, moreover, confirm the favourable effect of dry food upon the permeability of the pylorus, and in checking vomiting and cardialgia, in four cases of carcinomatous stricture of the pylorus.) I obtained, lastly, the same good results from systematic treatment with sulphate of soda and the use of a diet which resisted fermentation.

We see, therefore, that the same results may be obtained in different ways. The similarity of value, therapeutically, of such different methods of treatment is easily intelligible, if we fix with precision the therapeutical indications from a point of view common to all of them, and administer only such alimentary materials as are easy of digestion, small in bulk, and resist fermentation, permitting, at the same time, no retention of the ingesta, the consequences of which are fermentation, catarrh of the mucous membrane, gastrectasis and muscular atony, but causing the stomach to be every day regularly emptied in an upward or downward direction. The stricture, unless very considerable, does not imply the existence of serious danger, but of the above mentioned group of troubles. The consideration that it is possible often to guard against some of these and to remove others, makes the treatment of cicatricial stricture of the pylorus appear less hopeless than may at first sight have been the case. In particular, strictures whose extent is but slight, offer a very grateful field for the efforts of the physician. I can trace with interest, for a long series of years, many cases of narrowing of the pylorus, in which the troublesome symptoms occur only periodically. For months the patients are free from uneasiness, their stomach digestion goes on quite normally, until, in consequence of a perhaps only insignificant transgression in the matter of diet, a severe gastric catarrh sets in, and this is accompanied by serious indisposition. These exacerbations are characterised by great pyrosis; rarely, in the place of this, we have more profuse vomiting of acid fluids, frequent belching up of inodorous gases, and distressing pains in the stomach, with griping and tension, these being generally due to great dilatation of the stomach, and the action of its corrosively acid contents, and, lastly, to constipation. This entire series of symptoms points to a closure of the pylorus, which may be due, in part, to excessive tumefaction of the mucous membrane, and, in part, to a reflex spasm of the pyloric sphincter, and occasioned, as far as they are concerned, by the accumulation of the ingesta and distension of the stomach. The principal complaint refers to the heartburn, the pyrosis. "All that I eat or drink turns to vinegar." These and suchlike complaints are often heard. This distressing condition becomes permanent under the influence of unsuitable diet, whereas it rapidly yields to a rational treatment. In addition to a methodical course of the above mentioned alkalies, the best effects may be produced by an absolute abstinence from food, lasting for one or two days, after which the patient may take cold roast meat or raw ham, with some dry roll and claret. The pyrosis, the cardialgia, the vomiting, and the other symptoms disappear rapidly, as if by magic, with the occurrence of copious watery stools.

If, however, the pylorus is not permeable, in spite of the sulphate of soda, the best plan is at once to have recourse to the stomach-pump, and, with all needful precautions, to empty the stomach, and to wash it out with Vichy, Bilin, Giesshübel, or artificial soda water. If, after one or two such emptyings, the effect of the salts is re-established, the pump may be laid aside.

A certain disposition to troubles of stomach digestion, moreover, remains after the cicatrisation of ulcers which were not situated in the neighbourhood of the pylorus. This sensitiveness of the stomach to slight noxious influences may depend upon its limited mobility, due to adhesion of the cicatrised part to neighbouring organs, or to atony of the muscular tissue or glandular apparatus. We must always make it our business to remove this tendency to cardialgia, dyspepsia, and catarrh. I found a methodical course of the slightly ferruginous sulphate of soda water of Franzensbad and Elster to be very efficacious in such cases (I have no experience of the Kniebis baths); in many cases, on the other hand, the bitter waters, with a little iron (in addition to rhubarb as an eccoprotic), have a better effect; more obstinate forms always require a visit to Karlsbad, and there are many cases which we cannot avoid sending year after year to Karlsbad or Franzensbad. The plan to be followed out depends, as a matter of course, very much upon the individuality of the case, upon the external conditions of the patients, and so on. I may here offer a remark based on my own experience, that, with these conditions of debility of digestion and anæmia, iron, and the pure iron waters, are but seldom useful, and are generally badly borne. At any rate, when it is desired to make trial of iron, we must proceed very cautiously, and be particularly careful that the bowels are kept open.

The habitual constipation, which generally lasts for months, or even years after the ulcer has healed, must be combated very persistently, or else the appetite of the patient, and his bodily and mental vigour, will be liable to frequent and troublesome variations. For this purpose, the best remedy is rhubarb; but the preparation must be taken regularly every evening at bedtime. Three or four grains of the simple extract of rhubarb, or of the compound extract, when the constipation is more severe, is a sufficient dose, to which, if the medicine causes much pain, or the case is one of great atony, we may add with advantage from one-sixth to one-third of a grain of extract of belladonna, or alcoholic extract of nux vomica. As a general rule, these eccoprotics become unnecessary a few months after the complete healing of the ulcer, as likewise after that of the peritonitis, inasmuch as the obstacles to movement, as I have previously intimated, gradually become less, or may entirely disappear, and the glands of the stomach and its muscular coat may again acquire their normal energy.

A certain vulnerability of the mucous membrane of the stomach remains, however, in most patients, and compels them to take scrupulous, uninterrupted precautions with regard to diet; the stomach is to them always a *locus minoris resistentiæ* —a great trouble to those deficient in the self-control which is necessary, and to whom the pleasures of the table are matters of great importance.

ON THE

ARTIFICIAL EMPTYING OF BLOOD-VESSELS IN OPERATIONS.

BY

PROFESSOR F. ESMARCH.

GENTLEMEN,—You all witnessed yesterday a difficult and tedious operation, in which the patient lost a very large quantity of blood, in spite of all the care that was taken to prevent it.

The operation was one for the extirpation of a vascular, medullary-sarcomatous tumour, the size of a child's head, which occupied the whole of the upper part of the neck on the right side. It was found that the tumour had involved not only a part of the parotid gland, but also the neighbouring muscles, the sterno-cleido-mastoideus, the mylo-hyoideus, and the posterior belly of the digastricus, to such an extent that I found myself compelled to remove considerable portions of all these parts, so that, when the operation was completed, the internal jugular vein and carotid artery lay bare in the wound to a considerable length.

What, more than all, rendered the operation difficult, was the profuse hæmorrhage. You will remember that, with almost every incision, although I took care to make them as slight as possible, one or more arteries spirted, or veins poured out their dark blood over the field of the operation. You saw how I sought to check the hæmorrhage as much as possible by taking up the bleeding vessels, after each incision, with bull-dog forceps, and left these hanging in the wound while I went on with the operation. More than once there were hanging in the wound all the twenty-four pairs of forceps which I always have at hand in great operations, and I was compelled first to tie the vessels already divided before I could cut deeper. When the operation was at last finished, I had applied altogether more than fifty ligatures, of which, however, fifteen were applied on the tumour itself, so that only thirty-five remained in the wound.

I cannot make any guess as to the exact quantity of blood lost, since it was constantly removed with sponges; but we could judge that the patient had very little blood left in her body by the wax-like paleness of the skin, the small, weak pulse, and the laboured respiration.

Most of you will, no doubt, have said to yourselves that you would not desire to commence your career as operators with such an extirpation. And, in fact, it is just the "demoniac" blood, as Dieffenbach called it, which not unfrequently deters the young practitioner from performing important operations, especially when he cannot command sufficient and reliable assistance. And yet he only becomes a good operator who has learnt calmly to enter into the struggle with hæmorrhage. I need not explain to you here how important the question of hæmorrhage is in almost every operation. In many cases the limit we put to our operative undertakings is determined by the extent of the hæmorrhage to be expected. We do not venture to undertake many operations against which no other contra-indication exists, because the operation would last so long that we can foresee that the patient would bleed to death before it was completed, or because we consider him already too weak to survive the unavoidable loss of blood.

I shall perform an operation to-day in which the loss of blood would be still greater than in that of yesterday, if I did not adopt a procedure before commencing it which enables us to prevent the hæmorrhage entirely. In the patient about to be placed upon the operating table, there is almost total necrosis of both tibiæ, resulting from an acute osteo-myelitis, which followed a severe cold more than twenty years ago. You see that on the anterior surface of both legs numerous fistular openings exist, which discharge a large quantity of pus, and through which the probe comes everywhere upon roughened, moveable bone. On handling the legs, you feel that the bones are enormously thickened, and from the long standing of the disease it may safely be assumed that the thickened bones, the case which contains the dead portion of bone (sequestrum), must also be of considerable hardness. The position of the fistulæ, which you see extending on both legs almost from the upper to the lower epiphyses, justifies us in concluding that large portions of both diaphyses are dead, and the different depths at which sounds introduced into the fistulous openings come upon dead bone, indicate that the death of the bone at the different points occurred at different depths. If I leave a sound in each of these fistulous openings, and make a varying pressure with the upper sound upon the sequestrum, you see how all the other sounds are set in motion, and may fairly conclude therefrom that the entire sequestrum is moveable, and forms one continuous whole. To remove this, it is necessary to open the thickened bony case which contains it in its whole extent; and to ensure the complete healing of the large wound, I think it best to convert the bony cavity into a broad trough, by taking away the whole anterior wall, so that no adjacent cavities may remain to retard the healing process.

Those amongst you who have already seen similar operations will remember what profuse hæmorrhage accompanied them, and how greatly the performance of them was rendered difficult and protracted by the loss of blood. Our patient is still tolerably well nourished, and not exactly to be called anæmic; but I do not believe that I should have ventured formerly to undertake both operations at one sitting, because I should have feared that the loss of blood would have placed the life of the patient in great danger. With the aid of the process which I am about to show you, I do not hesitate to undertake both the operations simultaneously, and to spare the patient thereby a second operation, and a second long confinement to bed. My assistant, Dr. Petersen, will operate upon the right leg at the same time and in the same manner as I do on the left. While the patient is being put under the influence of chloroform, the leg is first wrapped in waterproof varnished silk-paper, to prevent the bandages from being soiled by the discharge from the fistulous openings; both legs are then firmly bandaged from the points of the toes to above the knee with these elastic bandages, which are made of woven indiarubber, the uniform compression from which drives the blood out of the vessels of the limb. Immediately above the knee, where the bandage ends, we now apply this indiarubber tubing, well drawn out, four or five times round the thigh, and connect one end with the other by means of a hook and brass chain attached to them respectively. The indiarubber tubing so thoroughly compresses all the soft parts, including the arteries, that not a drop of blood can enter the part so treated. This has the special advantage over the tourniquet, that we can apply it at any part of the limb, and need not be concerned about the position of the main artery. Even in the most muscular and stoutest individuals we are able thoroughly to control the supply of blood by this simple process.

We now remove the bandages first applied, together with the varnished silk-paper, and you see that both legs below the tubing resemble completely those of a corpse, and with their pale colour contrast almost uncomfortably with the rosy colour of the rest of the surface of the body. You will observe, also, that we operate precisely *as in the dead subject.*

We both now divide the soft parts along the whole anterior surface of the tibia down to the bone; a few drops of blood ooze from the bone and are wiped away with a sponge. From that time no more blood is seen. The periosteum, divided in the long direction, is now pushed back so far on both sides that the whole anterior surface of the thickened, uneven bone, with its numerous fistulous openings, is freely exposed.

We now take large chisels with wooden handles, such as are used by cabinet-makers, apply the edge to the uppermost fistulous opening, and, by the aid of a wooden hammer, remove the whole anterior surface of the bone in large chips.

The bone is very hard, as I expected. The work is not easy, and requires some practice, which you can best acquire in a joiner's shop. I must beg of you all to take care of your eyes, because the sharp and pointed splinters fly about with great force in all directions. We might remove this bony wall in another manner, with pannel saws, or Heine's osteotome; but this is so much more laborious and tedious that I greatly prefer the chisel.

The large sequestrum now comes gradually more and more into sight. You can easily distinguish it, by its whitish colour, from the reddish, living bone. The difference of colour is, no doubt, much greater if you operate without cutting off the supply of blood to the parts; the blood then streams out of all the pores which you see on each newly-formed surface, as if from a sponge, sometimes in strong jets, and so completely fills the cavity of the wound at each blow of the chisel, that you recognise nothing more, and cannot use the chisel again until your assistant has thoroughly mopped out the cavity with sponges. But now I require no assistant; my assistant, Dr. Petersen, chisels out his cavity, like myself, in the sweat of his brow. And now the hardest work is done. Both sequestra are exposed to their whole extent; we seize them with powerful forceps and draw them out with some exertion, because they send a few irregular processes into lateral cavities.

You see that the large, trough-shaped cavities in the bones in which the sequestra lay are partly lined with pale-red granulations. We remove these with a sponge, which we press firmly upon and rub over the uneven, bony surface, and with small, sharp scoops, with which we penetrate into the inlets and lateral cavities. We remove the granulations because they are, in my opinion, of no value for the reproduction of bone; they had been injured somewhat during the operation, so that they must have perished afterwards. You will be able to see, later on, that the whole surface of the bone very rapidly forms new luxuriant granulations, which soon become converted into bone-tissue and replace the great loss of substance.

The operation is now finished. We wash out the raw cavities with dilute carbolic acid, to destroy any decomposed organisms which may have got in, then put in some pieces of gauze steeped in a solution of chloride of iron, so as to cover the walls therewith, and now fill up both large cavities firmly with German tinder to above the level of the external skin. By bandaging with a strip of gauze dipped into carbolised oil, each of these pads is pressed well in; over this comes a layer of varnished silk-paper, air-tight round the whole leg, which is to be kept in its place with a common bandage.

We now, first, slowly remove the compressing indiarubber tubing. You see how the pale skin of the foot becomes red, first in spots, then uniformly everywhere, and soon even presents a darker red colour than the other parts of the body. Observe the dressing of the wound under the transparent paper; you nowhere see blood oozing through the gauze bandage. The patient has, therefore, not lost more than a teaspoonful of blood.

And now, observe the still quietly sleeping patient; he has, even now, the same red cheeks as before the operation, his pulse is full and strong, and the recovery in his case will, no doubt, be much quicker and more certain than if we had removed the bone in the usual manner.*

If we now compare the operation of to-day with that of yesterday, it will at once be evident to you how great are the advantages of this mode of proceeding, as well for the patient as for the operator. You have seen that we could both perform without assistance an operation difficult in itself, and will be convinced that this method will be very useful in ordinary practice, in which skilled assistance is often wanting.

You can adopt this method in almost all operations on the extremities with more or less complete success. In extirpations of tumours, tying of arteries, scraping off of scrofulous ulcers and carious bones, and in resections of smaller bones and joints, you can proceed in exactly the same manner as I have just shown you—*i.e.* you need not relax the compressing tubing until the dressing of the wound is completely finished.

I will now call your attention to another great advantage of this method. It consists therein that, in doubtful cases, where it is a question which is healthy, which diseased tissue, how far our active interference must extend, whether we must amputate or endeavour to proceed conservatively, resect, scrape off, etc., we are enabled by this method to examine the diseased parts much more minutely than would otherwise be possible. In many cases in which I was obliged to propose to the patients an amputation of the leg above the ankle, on account of disease in the ankle-joint, I have promised them, before administering chloroform, first to examine the diseased parts very minutely, and to save the foot if I thought it possible to do so. After driving out the blood as above, I have treated the foot exactly as on the dissecting-room table; have laid free and examined minutely the diseased bones and joints, and only performed the amputation when I had convinced myself that it was impossible to preserve the part, without the patient's having lost a drop of blood until I had tied all the arteries visible to the naked eye. Then only have I removed the tubing and tied the smaller arteries from which blood came.

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* The bandages were left on until the fourth day; when they were taken off, the enormous cavities already showed everywhere the first commencement of granulations; these were dressed at first with oil, and after some days, with an ointment of sulphate of zinc, and the healing process went on so rapidly, without any unfavourable accidents, that the patient was discharged, at his own request, as early as the 21st day.

I must observe generally, that amputations and larger resections of the joints cannot be carried out so completely without loss of blood, as in the operation you have just witnessed, because we must ensure ourselves against after-hæmorrhage before we dress the wound. If you amputate, you must relax the tubing as soon as you have taken up all the arteries which you can recognise as such with the naked eye. The blood now rushes at first with great force into the vessels and flows for a moment, as if out of a sponge, over the whole surface of the wound. Very soon, however, you distinguish single spirting arteries, and when you have taken up these you are pretty safe from after-hæmorrhage, because, from the dilatation of vessels caused by the removal of the tubing, even the smallest arteries show themselves at once, and no branch of any size can easily be overlooked.

In no case is the loss of blood great, and the results of amputation here have been extremely favourable since I employed the method described above.

As in the extremities, so you can entirely cut off the supply of blood to the male genital organs by means of indiarubber tubing. If you wish to remove a testicle or to amputate the penis, you apply a thin indiarubber tubing from behind round the root of the scrotum and penis, cross the ends in front on the mons veneris, and tie them on the loins. In this way I have often performed castration, as well as amputation of the penis, without losing more than the modicum of blood which is to be found in those organs before the commencement of the operation. If you would avoid even this, you must first apply narrow elastic bandages carefully to the parts, which would be very desirable in the case of large tumours of the scrotum. For slighter operations upon the prepuce or glans, it suffices to place once round the root of the penis a piece of very thin indiarubber tubing, such as is used for draining wounds and abscesses.

If we now cast a glance upon the historical development of our method, you will be aware that the endeavours to check, as far as possible, the loss of blood in operations are as old as surgery itself. They have made their mark in various ways upon the different periods of that art. If, in old days, limbs were amputated with red-hot knives, or the stumps dipped into melted pitch after the operation, this was done to become master of the hæmorrhage, because they did not know how to check it otherwise. Only when Ambrose Parey came upon the idea of stopping the hæmorrhage by taking up the vessels, did operative surgery assume a less discouraging character; that surgeon also gave instructions for limiting the loss of blood by strapping the limb above the point of amputation, and thus suggested the idea of the numerous methods of compressing the arteries, and of the invention of innumerable apparatuses for avoiding the loss of blood in amputations. That all these methods and all apparatuses are deficient, is proved most plainly by the circumstance that no method has attained exclusive precedence, and that new tourniquets are constantly being invented and recommended, of which not one has, in recent times, been employed by the majority of surgeons. I, at least, have not, while a student, seen a single amputation performed with the aid of the tourniquet. My teachers preferred having the main artery of the limb compressed with the fingers, which was considered as certain as the tourniquet, and, moreover, furnished the students and assistants with a desirable opportunity of practising themselves in the control of the hæmorrhage. It had gone quite out of fashion to use a tourniquet, although the patient often lost much more blood therefrom, especially if the operation lasted rather long.

For some time surgeons, therefore, sought to acquire a reputation for performing amputations as quickly as possible. C. I. M. Langenbeck, of Göttingen, was regarded as one of the most rapid operators of his time. He knew how to take off a leg or an arm with incredible rapidity by his oval method. When I was a student in Göttingen, an anecdote was related of him which affords a striking proof of this. A celebrated old surgeon once came to Göttingen to witness one of his rapid operations, and Langenbeck had promised to exarticulate an upper arm by his method. When the operation was about to be commenced, the old surgeon first turned round for the purpose of taking a pinch of snuff, but when he turned back again, the operation, to his great regret, was already completed. This surprise might have been equally well effected by his celebrated nephew, Bernard von Langenbeck, who, as General Staff-Surgeon of our Schleswig-Holstein army in 1848, astonished the foreign army surgeons by the incredible rapidity with which he performed his amputations.

This haste in the performance of operations resulted also partly from the wish to give the patient as little pain as possible, and since this object is now attained much more thoroughly by means of anæsthetics, the same importance is no longer attached to rapidity as formerly.

One of the most important tasks of the surgeon has always appeared to me to be the dealing as economically as possible with the "precious fluid" of the patients committed to his charge; and I began, as early as 1855, to swathe firmly with woven linen bandages, before an amputation, the limb about to be taken off, so as to press out, as far as possible, the blood circulating in it. The inducement to do this arose from an amputation of the thigh, which I had undertaken on account of a very large osteo-sarcoma. On proceeding, after the amputation, to examine more minutely the parts removed, I was shocked at the large quantity of blood which had subsequently run out of their vessels, and said to myself that such blood must, for the future, be saved. I recalled to mind, at the same time, an operation in which I had some years before assisted my predecessor, Stromeyer. It was the tying of the brachial artery for an aneurism, in which Stromeyer, to check the reflux of blood from the capillary vessels, first bandaged the fore-arm firmly as far as the aneurism before he applied a tourniquet above it. We had talked much on that occasion of the interesting circumstance that the blood which had been driven back out of the capillary vessels into the arteries, presented the dark colour of venous blood, and were astonished at the ease with which the tying of the brachial artery was accomplished after all the blood remaining in the arm had flowed from the incision.* No further conclusions for other operations were drawn at that time either by Stromeyer or myself from this observation, but I afterwards adopted this idea for amputations and exarticulations, and have since then always employed that method when I wished to spare the patient as much loss of blood as possible. I have shown that method to many surgeons, both in my hospital practice and in the many wars in which I was actively employed as consulting surgeon; and while attached to the Berlin barrack hospital, in the years 1870 and 1871, I always desired the surgeons to whom I was called upon to give counsel, to bandage the respective limb carefully before every amputation. In this manner, and with the aid of a compressor aortæ, I have succeeded in performing even exarticulation of the hip-joint with very slight loss of blood; but the process always remained incomplete, partly because I carried the bandages as far as the diseased part only, or, at most, to the point of amputation, and especially because I had the main artery compressed by the fingers only. In cases where the patient was very anæmic, and where the chief object was to lose as little blood as possible, I sought the welfare of the patient in performing the operation as rapidly as I could. Thus, for instance, I have frequently made the one-sided circular incision, which, of all the modes of amputation, may be completed most rapidly, in such a manner that I divided the whole of the soft parts with a single stroke of the knife down to the bone, then quickly sawed through the bone on a level with the incision, and took up the vessels. As soon as the hæmorrhage was checked, I leisurely pushed back the periosteum from the bone, so far as was necessary for sawing off a fresh piece of bone, several inches in length. Thus have I also, in a case of exarticulation of the thigh, endeavoured to reduce the loss of blood to a minimum by first taking up the vessels of the thigh *en bloc*, after forming the anterior flap, then dividing the remaining muscles by a circular incision, and rapidly sawing through the bone on a level with the divided muscles. All the vessels were now taken up singly, and then only was the upper end of the bone dissected out and exarticulated. In one such case, I even injected the blood poured out during the operation, after it had been defibrinated, immediately back into the femoral vein.

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* See Stromeyer's "Military Surgery," 2nd Edition, p. 164.

But in desperate cases all these measures leave us in the lurch, the loss of blood becomes greater than the weakened organism can bear, and thus the complete seclusion of the circulating blood from the parts to be operated upon continued to be greatly to be desired. For the extirpation of nævi occupying the whole thickness of the cheek, a method was already recommended by Dieffenbach, by which the access of blood can be completely prevented, until the application of the sutures at least. He made use of forceps whose arms terminated in oval rings, within which the tumour to be removed was firmly compressed. The compressor forceps, described by Desmarres and Snellen, for the extirpation of tumours upon the eyelids, and which are also very useful for operations on the lips, act in a similar manner. I use myself, in cases of extirpation of nævi, etc., rings of horn or tin, which are pressed by the fingers of an assistant firmly round the tumour, and generally entirely prevent the access of blood, especially when they meet with a firm basis, such as the bones of the skull. To discover a similar method for greater operations has long appeared to me a thing much to be desired.

With the so manifold and successful employment of indiarubber in surgery, it lay near at hand to use its elasticity for our present purpose, and it proved efficient beyond all expectation. As soon as it was ascertained that we could very easily completely interrupt the circulation with an ordinary indiarubber tubing the present method developed itself very quickly.

Tubing, such as is used for counter-extension in the weight-treatment of inflamed joints, served at first as a tourniquet, and one of those indiarubber bandages with which we are enabled so rapidly to disperse dropsical effusions into the knee-joint, was used for bandaging the extremity. With every trial I made of the new method its advantages became more apparent. One improvement was tried after the other. One of your fellow-students undertook to study the physical and physiological circumstances experimentally;* and the more I became convinced of the great advantages of the method, the more desirous I was to extend it to as many operations as possible. The field is, unfortunately, a very narrow one. We can prevent the access of blood completely in the extremitics only, and external organs of generation in the male.

But the tubing may perhaps be found useful in operations on the trunk, neck, and head, by shutting off the blood of all, or some of the extremities from the general circulation, by strapping, and thus forming reserve stores from which we could admit the blood successively again into the general circulation if the patient was in danger of bleeding to death. This is, however, an idea only, the practicability of which careful experiments on men and animals can alone determine.

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* Dr. Iversen: "Artificial Ischæmia in Operations." Inaugural Dissertation. Kiel, 1873.

I cherish the hope, however, that this method may yet be available in many directions, but must, in conclusion, ask one question which is of the greatest importance with regard to its introduction, namely, whether dangers may not arise to the health of the patient from the employment of it. In any case, we must not ignore the possibility that the firm strapping of a limb for any considerable length of time may be followed by dangerous derangements of the circulation and innervation, such as thromboses, inflammations, paralyses, etc.

After the thousandfold experiences, however, which the surgeons of all times and of all countries have had in the employment of the tourniquet and of compression with the fingers, it was not exactly probable that an interruption of the circulation, even if complete, could be followed by such evil consequences, unless continued for a great length of time. And if the carefully performed experiments of Cohnheim have shown that in warm-blooded animals, the total interruption of the circulation of the blood is not in general followed by any permanent disturbance, if it does not continue more than six or eight hours, I may now, after having performed during the last year more than eighty operations on parts artificially emptied of their blood, assure you that I also have never seen such disturbances ensue which could be pointed out as consequences of the method in question. I have performed operations which lasted more than an hour, and have not found that, during the recovery, any disturbances of the circulation showed themselves; on the other hand, it is seen that the wounds made during the operation ran a strikingly favourable course since I have adopted this method, and accidental, traumatic affections occur only exceptionally.* There is one precaution which I would urge

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* From the beginning of this year to the 15th of August this method was adopted in eighty-seven operations out of 329.

Of these eighty-seven cases, twenty-one were amputations and exarticulations, amongst which were six amputations of the thigh, eight of the leg, one exarticulation of the upper arm, eight resections, thirteen operations for removal of dead bone, five extirpations of tumours; the rest, slighter operayou strongly to take when adopting this method. If you are operating upon parts infiltrated with ichorous matter, you must refrain from emptying them completely of their blood. If you bandaged such soft parts tightly, you would be in danger of driving the infectious matters into the meshes of the cellular tissue and extremities of the lymphatic vessels, and might possibly do much harm thereby.

In such cases I do not put on the bandage at all, but content myself, before applying the tubing, with emptying the limb as completely as possible of blood, by causing it to be raised high in the air for a few moments.

tions, such as the scraping out of carious bones, or scrofulous and lupoid ulcers, opening of extensive abscesses and burrowings under the skin, circumcisions, etc.

Of these eighty-seven patients, only four have died. Most of the amputation stumps healed by the first intention, and almost without any traumatic fever.

ON

LUPUS AND ITS TREATMENT.

BY

PROF. RICHARD VOLKMANN.

GENTLEMEN,—I show you to-day four patients suffering from lupus, recently admitted, and you may conclude therefrom, what is already sufficiently well known to those amongst you who have been longest here, that this painful affection is by no means rare with us. The rural population, especially, furnish us with a considerable contingent, while in the town lupus certainly occurs much less frequently. Two of the patients are children; the third is a girl who has just reached the age of puberty. This is the age at which, according to my experience, this disease by far most frequently occurs, and in more than half the number of cases, between the ages of 9 and 15. The fourth case is that of a woman, aged 59, a period of life at which lupus has generally either ceased spontaneously, or been cured by treatment, and but rarely comes under our notice. The disease in this latter case commenced when the patient was 10 years old, so that we have before us an affection of nearly fifty years' standing. The ravages it has committed are in proportion to its long continuance.

The patient to whom I beg first to direct your attention is this girl of 12 years of age, with hypertrophic lupus on the back of the hand and foot on the left side. You see how the back of the hand, as far as the first phalanges, has been converted into a bluish-red, knobby, in some places almost warty mass, which rises a quarter of an inch above the healthy skin. This surface is covered partly with thick crusts, partly with open ulcers, and a very similar condition of things is to be seen on the foot. Not the least doubt can exist concerning the nature of the lesion if you examine closely the edges of the diseased portions of the skin, for it is evident that the new formation arises everywhere from the confluence of small brownish-red nodules on a level with the skin, which is so characteristic of lupus.

It so happens that I have known this patient from her earliest childhood, although I have entirely lost sight of her for the last few years. I treated her for a very serious affection of the ulna, also on the left side, when she was only two years old. A piece of bone of the size of a cherry, consisting of the much thickened, spongy portion, covered with crumbling, cheesy matter, became slowly detached from the posterior surface of the superior epiphysis of that bone. But although the process went on in the immediate neighbourhood of the elbow-joint, and even a portion of the olecranon was also lost, the cavity of the joint remained free, and nothing is now to be seen beyond a considerable defect in the bone, and deep cicatrix. Later on, the child suffered much from swellings and chronic suppuration of the sub-maxillary and cervical glands, of which there now remains a broad strip, consisting partly of cicatrices, partly of sinuous ulcers, which extends round the chin from one ear to the other.

With these antecedents, we must, in the language of the day, regard the child as scrofulous; and this gives me the opportunity of explaining to you my views concerning the relations of lupus to scrofula, as well as to other constitutional diseases.

As regards the former, Virchow has quite recently denied entirely the connection of lupus with it; while the first French dermatologist, Bazin, has distinctly pointed out the affection with which we are now occupied as *malignant scrofulide.*

In my opinion, both these views are too one-sided and disproved by experience. I have frequently been unable in practice to confirm Virchow's assertion that affections of the lymphatic glands in a distinct form "are always wanting." Thus we have at this moment in the clinical wards two cases of ulcerative lupus of the nose of moderate extent, in which hypertrophy and suppuration of the lymphatic glands, partly of the lower jaw, partly of the parotid, exist. One of these you see in this boy of nine years old. In the case of a third patient, in whom, after curing the lupus, we undertook the rhinoplastic operation by dissecting off and transferring the skin of the root of the nose and glabella, and who is in attendance here at the present time, I was obliged to extirpate a large complexus of hyperplastic or cheesy lymphatic glands from the angle of the jaw.

Cases of chronic conjunctivitis, also, with obstinate photophobia, or multiple ulcers in various parts of the body, with undermining of the skin, not unfrequently come under my observation in persons suffering from lupus; and a small number of my patients have died, later on, of tubercular disease. According to my experience, patients in whom the disease has attacked the upper extremities are the most disposed to tuberculosis, and chronic suppurations of the skin or bones in these parts seem to stand in close relation with tuberculosis. The difference between the upper and lower extremities in this respect is a very considerable one, if we compare equally large numbers of cases in both localities. On the other hand, after equivalent affections of the lower extremities, parenchymatous nephritis is of much more frequent occurrence. The trunk occupies an intermediate position, and inclines with equal frequency to each of these forms of secondary affection.

This by the way only. Still much less is it justifiable to set down lupus, without further examination, as "scrofulous." It is always an exception when the characteristic affection of the glands appears; an exception when suppurations or chronic inflammations of other parts of the body are superadded; a still greater exception, lastly, when patients die of tubercular disease. In ordinary cases, nothing of all this is to be observed. In spite of a duration of the morbid process for years, the lymphatic glands remain uninvolved, and no further infection of the body occurs, as so often happens from the local nests in cases of well-marked scrofulosis. A comparatively large number of lupus patients are, and continue to be, rather well-nourished, thick-set individuals, enjoying excellent health, in whom nothing whatever like dyscrasia, or any special diathesis, is recognisable.

Virchow, in his excellent description of the lupoid process, in his great work on tumours, has made one short observation which I cannot leave unnoticed here. He points out that certain forms of tumor albus of the joints present an extreme resemblance to lupus, and that a special examination is required to determine whether we should not be justified in calling them directly Lupus articularis.

You will, perhaps, think that this author, of whom we have just heard that he utterly repudiates the idea of a scrofulous lupus, is here in contradiction with himself, inasmuch as the near relations between tumor albus and scrofulosis are generally admitted. This is, however, by no means the case, and I must confess that I find his remark very appropriate. But it is especially a variety of the so-called fungous inflammations of the joints—running its course with very little or no suppuration, and yet of a highly destructive character, and in which, with absence of fever and undisturbed general health, the epiphyses are reduced by hard granulations to small, irregular, pitted stumps, and cicatricial contractions of these granular masses, and, as I have shown, even great contractions about the bone very soon set in—which presents a striking resemblance to lupus. But this so-called caries sicca of the joints occurs, just like lupus, mostly in otherwise healthy, well-nourished persons. Only very exceptionally does it coincide with the scrofulous diathesis.

On the other hand, I quite agree with Virchow in repudiating altogether the connection of lupus with syphilis, whether acquired or congenital. As regards the former, most authors are now agreed; but as regards the latter, I can assure you that, in cases in which its existence must necessarily be admitted, I never saw, in the face at least, forms of ulcerative inflammation of the skin which did not differ altogether from lupus. On the other hand, I have never been able to detect in a case of lupus synchronous phenomena of hereditary syphilis.

Moreover, the diagnosis of lupus is in general a very clear one; the whole form and external development of the disease thoroughly typical. In the face, where lupus most frequently commits its ravages, you will scarcely meet with a single case which leaves any doubt as to the nature of the disease. At most there are cases of malignant eczema, especially in front of the ear, in the neighbourhood of the parotid, which, running their course with strong burrowing granulations in the skin, and consequent cicatricial contraction of the parts, furnish transitions to lupus. They may be designated Eczema luposum. On the extremities cases are, no doubt, sometimes observed in which we hesitate, not only whether to class them with lupus or syphilis, but with scrofulosis or even lepra. These also are, however, rare.

A similar state of things is present in lupus of the mucous membranes. We here meet with unmistakeable cases of primary disease of the conjunctiva, the mucous membrane of the nose, the gums and throat, perhaps even of the larynx. But frequently as lupus passes from the outer skin and transitional points of the orifices to the mucous membranes, it very rarely commences primarily in them. Scrofulous ulcerations (nose, gums, and throat), diffused, warty excrescences resembling cancroid (larynx), which occasionally, in individual cases, assume a certain resemblance to lupus, are much more frequent in those situations.

Beyond these localities, lupus is scarcely found anywhere else but in the vulva. I have seen this only once here, so that I cannot speak from personal experience, but Huguier has described nine cases in that locality, and given very characteristic sketches of them.

In the person of the third patient, Gentlemen, I present to your notice a rather rare case.

You see a very robust woman, 59 years of age, whose face, from the forehead to the chin, and from one ear to the other, is partly disfigured by lupoid cicatrices, and partly covered with bluish-red nodes and superficial ulcers. The skin of the face is drawn tightly over the bones, thickened and shining; the left eyelid is ectropic, the nose destroyed. Large curves of fresh lupoid eruption surround the diseased portions of the face, towards the temples and ears, as well as towards the skin of the neck. The process is most active at the edges of the large diseased surface. This is, therefore, the form of lupus which is named *serpiginous.*

The disease commenced in this case when the patient was between 9 and 10 years old, and has gradually attained its present extent. It has sometimes remained quite stationary for months, or even years, after which new, violent outbreaks again occurred. Quite contrary to the general rule, and in spite of the patient's age, the disease has progressed most rapidly during the last fifteen years.

But all this does not include my reason for presenting this patient to you. Within the last year her condition, as she herself has observed, has entered upon an entirely new phase, on account of which, after the disease has existed for fifty years, she has determined to attend at the hospital. In the middle of the left cheek a node has formed half the size of a hen's egg, which is covered with closely adherent, thick crusts. Where these have become detached, a warty new formation, fissured in various directions, is observable, which bleeds readily, and from which, on pressure, thick, maggot-shaped plugs are expelled. When placed under the microscope, these are seen to consist of a pulpy mass of epidermis-cells in a state of fatty degeneration, in which may be seen a number of coherent, bung-shaped, or bulbous formations made up of horny, laminated epidermis-cells. At the same time this large node is harder than is ever met with in lupus. There can be no doubt that a cancroid, an epidermoid cancer, has here been developed out of the lupus. A similar condition of things obtains at the tip of the nose. Upon it there is a thick disc the size of a shilling, which presents an entirely similar fissured surface, and whose edges are already beginning to turn back, in the characteristic, ruffled manner, towards the remains of the root of the nose.

This also is a cancroid. Two cancroids developed synchronously from old cicatrices or ulcers of lupus!

This later transition from lupus to cancroid has already several times been observed by others, but is yet, on the whole, as already remarked, a very rare phenomenon. I may, therefore, regard it as an accident that the present is the third case which I have already seen. Both my other observations were also in elderly women. In both these cases the carcinoma developed itself in the upper lip and formed a large, flat cake, which spread to the root of the nose. Both were operated upon. The one patient was cured permanently; the other died from a return of the disease with cancroid infiltration of the glands of the lower jaw.

You will easily understand that this combination of lupus and cancroid has a more than mere clinical interest, it has also a certain histiogenetic interest. For, since a number of the earliest observers had already asserted that lupus arises from an exuberant growth of the cells of the rete Malpighii, so might we, at least, who uphold the theory of the epithelial origin of carcinoma, perhaps regard the gradual transition from the first-named affection to the second as a new and powerful argument for the equally epithelial nature of the lupoid new-growth. But, in like manner as we see cancer of the skin develope itself in old lupoid cicatrices, or even lupoid ulcers, so do we also see it arising from a long-standing ordinary ulcer on the foot, in an old bone fistula, in the cicatrix of a burn constantly reopening itself, or even in a seton wound. And the process is always a very similar one, as well in the two cases of lupus formerly observed by me as in the patient now before us. The disease attains a more warty middle stage, in which we do not well know what turn things will take; in which we are at one time in doubt whether it is not perhaps merely a simply irritative condition resembling elephantiasis; at another, whether it is not the hypertrophic form of lupus which is developing itself, until by degrees the state of things becomes clearer, and the softened epidermoid masses protrude on pressure between the warty growths, the infiltration in the vicinity becomes more massive and knotty, and the destruction extends deeper and deeper.

Moreover, the clinical course of lupus speaks entirely against such a near relationship to cancroid. The history of each individual case is made up of a long succession of alternating exacerbations and abatements, of new attacks and new recoveries. At single points the process remains stationary, and permanent cicatrisation occurs; while at others new outbreaks follow, and after these have lasted ten, fifteen, or even twenty years, a spontaneous cure takes place, even in the worst cases, with great regularity. We never see in the individual growths a continuous increase from a single nest; there are always multiple eruptions which, it is true, often run into each other, of which many simultaneously run a different course.

These are evidently the characters of a specific, chronic inflammatory process, such as we must necessarily acknowledge lupus to be, and it is not difficult to convince ourselves that this process commences essentially in the connective elements of the cutis, and not in the rete Malpighii, or glands of the skin. With very abundant new formation of vessels, which may give the diseased part a complete teleangiectatic character, the cutis at first becomes converted into separate, entirely circumscribed nests; then more diffusely into a layer of small cells, which, as Virchow was the first to show, does not differ essentially from common granular tissue. The cells have the form and size of the white elements of the blood, and often form immediately under the mucous layer, from which they are, however, always at first separated by a transparent covering of connective tissue, such sharply defined, large, round or oval masses, that we are at once reminded of miliary tubercles, or, at least, of the bodies resembling them which Köster has recently described in the fungous granulations of the synovial membrane in tumor albus. It is quite possible, therefore, that further resemblances between lupus and fungous inflammations of the joints may here become apparent.

Near these circumscribed nests, however, we always find places in which the exuberant lupoid growth proceeds diffusely and forms a network in the course of the vessels, with meshes which grow constantly smaller, and at last disappear. We have here forms which do not differ essentially from those which we find, for instance, when we examine the brownish-red, swollen, and shining skin of the lower leg in persons who have suffered from obstinate so-called varicose ulcers of the foot.

Lupus is thus to be classified with the new growths consisting of granular tissue. Characteristic of it is the so copious aggregation of the small exuberant cells, that the elements of the cutis, and, not unfrequently, of the still deeper-seated layers of cellular tissue, are completely displaced and destroyed by them. The infiltration very soon leads to a complete substitution, and, if the exuberant cells disappear, the result is a permanent loss of substance, which sometimes shows itself as a special defect, sometimes as a contraction of the parts previously diseased, and sometimes, lastly, as a scar.

You will be inclined to assume that these changes which, in combination, produce the serious disfigurations which lupus is wont to leave behind it, during the healing process, on the site of its earlier activity, are connected only with the ulcerating, or, as the older writers termed it, corroding form. This is, however, by no means the case. They occur also when the formation of ulcers has never been reached. The infiltrated, thickened parts go back after recovery, not only to their former volume, but below it. After simple exfoliating lupus, the tip of the nose becomes pointed, as if pulled off, the prominence of the alæ nasi disappears, ectropia, or distortions of the angles of the mouth occur, the skin appears to be interwoven with irregular, cicatricial lines, or even acquires an irregularly pitted surface. These are, therefore, processes which may remind you, more or less, of cirrhosis of the liver, or similar conditions of other internal organs. They occur, further, in a similar manner, almost exclusively in syphilis, which, while the layers of the skin remain quite unchanged, and no suppuration occurs, is capable of committing the most extensive ravages. You will never observe anything similar in scrofulosis. The cicatricial loss of substance occurring after lupus is, in so far, not entirely without importance for the diagnosis.

The fourth case which you have an opportunity of examining to-day appears, at first sight, to be so very different from the three first, that you will not be surprised to hear that it has only very recently been decided to classify this with the other forms of lupus. The patient, a young and very robust country girl, was operated upon here about a year ago for an encysted tumour of the upper jaw. She was at that time otherwise perfectly healthy. Within half a year, however, there have formed on both her cheeks and on her nose irregular, scarlet patches, with little swelling, which have now almost run together into a single connected figure. At single points the reddened skin is covered with very firmly attached, dirty-looking, very thin crusts, which, as you can easily ascertain, are distinctly fatty to the touch. They consist, in fact, of nothing further than an excessive secretion from the sebaceous glands of the skin, mixed with epidermis-cells. When we have succeeded, with great difficulty, in scraping off this fatty layer with the knife, the underlying skin appears red, sore, and as if studded with fine warts. But if you examine these warty points more closely with a glass, you see that it is by no means a question of papillary elevations, but of a large number of fine holes, which, being closely adjacent to each other, produce the warty appearance. These holes are the enlarged openings of the sebaceous ducts, and you can also see, on pulling off single fatty crusts, how a fine prolongation of the latter becomes detached from each small opening. In so far, it is a question, therefore, of a seborrhœa. On this account, Hebra had formerly termed this affection Seborrhœa congestiva, while the present customary name of Lupus erythematodes was first introduced by Cazenave. This name also is anything but a happy one, as regards the adjectival part, on which account I have preferred calling it "Seborrhagic lupus."

In any case it is certain that the seborrhœa is a condition of only secondary importance. The essential point here is the fine-celled infiltration of the cutis, which usually develops itself most freely in the immediate neighbourhood of the glandular inversions (*drüsige Einstülpungen*) of the epidermis, and gives rise to a hyperplastic condition, and increased secretion of the latter. This form is distinguished from Lupus vulgaris by the circumstance that the infiltration is less massive, more superficial and diffused, and less disposed to form nests. In other respects it undergoes the same transformations; and thus we see in our patient, in spite of the short duration of the disease, cicatricial retractions arising on the nose especially, but also at other points. The tip of the nose in particular has, in the short time, assumed quite a pock-marked appearance. These slightly pitted, granular cicatrices, with central retractions, are very characteristic of seborrhagic lupus.

If we now turn to the consideration of the treatment of lupus, we may safely assert that it has made immense progress within a few years. In a very great majority of cases we effect a cure with absolute certainty, and often require for it only as many weeks as formerly months or years. In justice we must admit that this is chiefly owing to the services rendered by Hebra. With him originated essentially the methodical surgical treatment of skin diseases, to the principles of which we now all give our assent.

The treatment of lupus is thus exclusively local. Internal specifics for it do not exist; concomitant constitutional derangements are, as you have already heard, only exceptionally demonstrable. Thus there is usually no indication for giving internal remedies. Even when a scrofulous diathesis exists, and a general, and especially a dietetic treatment, becomes necessary, you still cannot expect to exert any influence over the lupoid process without the simultaneous employment of local measures.

The local treatment has, however, two problems to solve.

The first is, to remove those tissues which are so intensely affected that new permanent tissue cannot again be formed from them, and which, if the patient were left to nature, could only be removed by a slow process of ulceration before a recovery could take place; the second, to effect in those parts which are still firm and comparatively healthy the absorption of the lupoid cellular infiltration, which is in some places diffused, and in others has a tendency to form nests.

We must first remove, in the one way or in the other, every, even the smallest, nodule of lupus; then everything which has become so soft that it can be pierced, or removed with blunt instruments, or which melts like butter under the stick of caustic. Anything having about the consistence of the normal skin must be preserved, especially all portions of skin with mere diffuse lupoid infiltration, and having a smooth, not ulcerated, though, perhaps, bright red and thickened surface. (Lupus non exedens, exfoliativus; Lupus erythematodes.) The indications for the choice of one or the other mode of proceeding are, therefore, very simple.

Lastly, every relapse is to be suppressed at once. In reference to this point we must explain the state of things very plainly to the patient, so that, should he be dismissed, he may present himself again on the appearance of the first red spot or nodule of lupus. If he does this, we have the matter entirely in our hands, and I have no hesitation, in the case of sensible and favourably situated patients, about performing any plastic operation which may become necessary very shortly after the cure of the lupoid affection. I undertake such operations for the restoration of lupoid defects many times in every term, and have never allowed the newly-formed part to be destroyed by lupus.

The means which have been recommended in various quarters for the attainment of these ends are very numerous. Many paths lead to Rome, and it is pretty generally admitted that more depends upon the way in which these various means are employed than upon the choice of the particular remedy. The treatment of lupus is one of the many chapters in our art in which much depends upon an exact technology and method. We have to learn from experience how to measure each remedy, with what intensity to let them act, and under what particular circumstances we should give preference to the one or the other. This can only be learnt in practice, not taught. You must not, therefore, expect to obtain such good results from a given mode of treatment, if you are employing it for the first time, as some one who has employed it for years, and is known to have been very successful with it. This logical error is, nevertheless, very often committed, and is the very common cause of the diversities of opinion unfortunately frequently met with in our science in different, equally capable observers concerning one and the same apparently simple mode of proceeding. One man has effected nothing with a certain remedy, another very much. Take care not to console yourselves, in such cases, with the commonplace phrase of good or bad luck, or of chance. It is a serious impediment to progress in our art, of which we must judge, on the whole, by the results obtained. If two surgeons, in a sufficiently large number of cases, treat diseases with external remedies, and direct, controllable applications, employed assumedly in a similar manner, and actually obtain quite different results, you may conclude with certainty, should no adverse influences, dependent upon local circumstances connected with the state of the hospitals in which the patients are treated, exist, that their modes of procedure have differed in some very important respect, and that the first thing to be done is, to make this point of difference clear. One of the greatest advantages of a "school" is to master, in their minutest details, certain modes of treatment of a teacher which he has ascertained, by the labour of years, to be especially efficient.

Before describing to you more in detail the mode of proceeding which I now adopt almost exclusively in the treatment of lupus, and then showing it to you in its actual employment in a patient, I will mention to you very briefly those modes of treatment which are in most general use. The majority of you will also see them employed occasionally as adjuvants in our wards.

For the removal of tissues utterly degenerated from lupus, caustics are generally employed.* Of course you may use one as well as another, if you only know how you are to employ it; and so there is, in fact, scarcely a single kind of caustic which has not its powerful advocates. Two are most frequently used and most applicable—*caustic potash* and *nitrate of silver*, both in the form of a stick. Firmly attached crusts may be removed before the caustic is applied. This is most easily affected by applying cod-liver oil to them for one or two days, which soon causes the crusts to become detached. You then bore the stick of nitrate of silver or caustic potash into the soft, lupoid granulations, which are easily penetrated thereby, and, where they come into contact with it, completely melt away.

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* Until lately I believed that no doubt existed amongst men of science as to the necessity of removing tissues completely softened by the lupoid process, if we expected a perfect cure. I see, to my surprise, that this is not the case. Küss, in Strasburg, only advocates the use of caustics in very

It is scarcely possible ever to do too much with the nitrate of silver, for which reason it is more to be recommended for the less experienced. You cannot penetrate the healthy, or almost healthy, cutis with it, even if you press pretty firmly. On the contrary, it is necessary to do this, especially if you wish to destroy nodules of lupus not yet in an ulcerating condition; you must introduce the pointed stick as deeply as possible into these, and turn it round. Again, it is impossible to produce with this remedy a scab extending deeper. Neither will a larger portion afterwards become necrosed and detached than you intended. Lastly, it is not of much consequence if some of the corrosive fluid from the melting stick runs into the mouth or nose. It will never act destructively, as under similar circumstances would always be the case with caustic potash, in the use of which special precautions are always necessary. These consist, for instance, in plugging the nostril, as far as it is healthy, firmly with lint, and in pressing pledgets of oiled lint upon the neighbouring parts, etc., etc.

This second caustic acts much more energetically. You must not here leave the stick long in contact with the parts to be cauterised, as you would otherwise destroy the underlying sound tissues, and produce large sloughing scabs. The lupoid tissue melts with the exuding blood quickly into a syrupy, tar-like mass, which must be at once wiped off with pads of

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superficial affections, and in severer cases advises that the diseased parts should be covered with adhesive plaister! His reason for not treating deep-seated lupus with caustics is certainly very ingenuous: "N'y touchez pas, car le terrain est si mauvais qu'à la moindre secousse le tout s'écroule." That is just what we want. Compare Mengus, Strasburg thesis, 1868.

wadding until a sound, firm surface appears. At this moment you must leave off. Every year I see a number of cases in which badly-instructed practitioners have caused such deep erosions in patients affected with lupus, that the most extensive and disfiguring scars were produced, and the plastic reparation of the deformity was rendered most difficult. It happens also not unfrequently that, in spite of this treatment, fresh lupus-nodules have again shown themselves between the hypertrophic cicatrices, so that the patient has not even gained any real advantage. Such deep cauterisations are not, in fact, of the least value. If you wished to destroy all the parts occupied by small-celled nests in persons affected with lupus, you would be obliged, in many cases, to remove a quarter or half the face without being ensured thereby against relapses in the remaining portions.

One advantage in the use of caustic potash is that the pain soon ceases, while it lasts much longer in the case of nitrate of silver. The inflammatory reaction, also, which follows the use of the former remedy is, notwithstanding the much deeper penetration, usually considerably less. This is not, however, to be regarded as an advantage. It is only desirable when the first application produces a very energetic, so to speak, traumatic reaction in the parts which are left and are still largely occupied by lupoid cells, which are thereby acted upon very favourably.

Caustic ointments of arsenic, formerly much in use, are getting more and more out of favour. I cannot recommend them to you. We cannot define the depth with which we wish to act with them so exactly as with a stick of caustic in our hands. Least of all in the case of strong ointments, when we wish to cauterise deeply. They are at the same time extremely painful, especially since the dressing with the particular kind of ointment always has to be applied for some days successively. Arsenic is most useful for superficial lupoid ulcers, *e.g.* in the form recommended by Hebra; Ars. alb., 1 oz., Cinnab., 3 oz., lard, 25 oz. The strength of the ointment is here so proportioned that the sound parts are not affected by it, but only the softened, lupoid parts. The diseased parts are dressed with this ointment for about three days successively, after which they are simply strapped up.

The actual cautery in its simple form was tried about thirty years ago and rejected. Neumann has, nevertheless, recently recommended the galvanic cautery for the removal of deeply degenerated lupoid parts, and speaks highly of its beneficial effects. There is certainly no reason to doubt that we may obtain good results with this remedy also, but we have no great cause for choosing a remedy of which therapeutical experiments, carried out for centuries in the most various parts of the body, have shown that it is extremely difficult to judge how deeply we go with it, and which, more than any other, so changes the tissues acted upon as to render further observation of them impossible during the operation itself. It is also well known that such wounds readily form very voluminous cicatrices.

Again, the excision of lupoid portions of skin by means of incisions in the sound parts is, in very exceptional cases only, either possible or desirable; *e. g.* either when, with perfectly circumscribed disease, the gap formed can be at once closed by suture, or when, through the excision, the conditions for an immediate plastic operation are rendered more favourable. Beyond this, excision is not advisable. The same may be said of it as of too deep, escharotic cauterisation. We sacrifice parts which we might save, and produce, unnecessarily, deep and voluminous, disfiguring cicatrices.

After removing, by one means or the other, all useless parts, we must proceed to attack the everywhere present, diffused, or nest-like infiltrations of the still firm and non-ulcerated, but more or less swelled and bluish-red coloured neighbourhood. The same must be done when the lupus has been from the beginning "non exedens, exfoliativus, maculosus," or however else it may be called; also in the erythematous, seborrhagic form. In all these cases, we have, moreover, generally to deal with affections spread over such large portions of the face that no one would think of destroying the diseased parts of the skin by means of caustic.

Here also very various remedies have been employed to act, as it was formerly called, "alteratively" upon the diseased skin. Most of them act by causing a violent inflammatory action of the skin, by means of which the imbedded lupoid-cells are probably brought partly to acute inflammatory fatty metamorphosis and consequent absorption, partly to organisation into permanent elements of tissue (connective tissue, cicatricial substance). As regards another series of remedies, we can form no idea of what nature may be the salutary processes brought about by their use.

To the first category belongs, again, especially nitrate of silver, then iodine; to the second, regulus of mercury.

Since nitrate of silver does not penetrate through the Malpighian layer as far as the cutis, and no losses of substance or cicatrices are produced by it, we may, in the cases of which we are now speaking, and even when the disease is of great extent, rub the implicated portions of skin firmly with the stick, and repeat this process freely at longer intervals. Concentrated solutions of the nitrate, which we apply with a brush, are, however, frequently more convenient. It is well, in such cases, to cleanse the skin carefully from grease, so that the aqueous solution may come better into contact with it. You may wash the skin for this purpose with spirits of soap, a strong solution of soda, or something similar, or employ Hebra's very useful "Spiritus saponato-kalinus" (Sapon. virid., 30 oz.; Sp. vin. rectif., 15 oz.; Sp. lavand., 5 oz.). This, if applied energetically, excites the skin pretty freely, and may, where the infiltration is very superficial, be used occasionally alone.

Iodine is used dissolved either in glycerine or alcohol. The much used, so-called Richter's (Hebra's) solution of iodine, consists of one part iodine, one part iodide of potassium, and two parts glycerine; a blackish-brown fluid of syrupy consistence. With this you paint the affected parts several days successively, morning and evening, until crusts are formed, covering the iodine with oiled silk to prevent evaporation, and when the crusts have become detached, repeat the process until the desired end has been attained. On the other hand, if you use alcoholic solutions of iodine, which I find in general more convenient, you must employ them of a strength of at least 1·8 to 1·6, and must paint the affected parts so frequently for several days successively as to produce a bluish-black colour. This is soon followed by a strong reaction, with formation of vesicles, and, later on, of crusts.

The employment of mercury, in the form of ung. hydrarg. ciner., in lupus, for rubbing-in or as dressing ointment, is far from new. Early in the present century it was especially recommended by Key (*Lancet*, vol. xv.), and that with an addition of ol. terebinth, æth., and camphor, which is not essential. It has been known to me since I first began to practice surgery through Blasius,* and both he and I have obtained a number of excellent results with it. Quite recently, Hebra and Moritz Kohn have also called attention to this remedy, which they appear to have overlooked until then, and they especially recommended the emplastr. mercuriale in the so often very obstinate lupus erythematodes. In fact, the results, symptomless as they take place, are sometimes very surprising, and the remedy itself is a very convenient one. Thus it is much employed in my clinical practice, partly as an adjuvant or in after treatment, partly for such out-patients as are not in a position to subject themselves to a regular clinical treatment. It may be employed for a very long time without interruption. Salivation ensues very rarely. One patient, a day-labourer from a distant village, with lupus of almost the entire face, in its worst form, did not present himself until a year and a half after gray ointment had first been ordered for him. He had rubbed in the ointment twice a day most conscientiously over the whole face during that long period without producing salivation. The lupus was almost entirely healed, with deep pits, it is true, but with scarcely any cicatrices.

I have since then made numerous experiments with gray ointment in other affections. As you are aware, it was formerly a remedy never to be omitted in the methodical antiphlogistic treatment of external inflammations, and always to be rubbed in at all phlegmonous points of the skin, on every acutely swollen lymphatic gland, etc. This custom has now passed away, and rightly so, for its value is at the least very problematical. Of much more use in certain cases is the ung. hydr. ciner. as a dressing for open wounds and ulcers, or for the severer forms of croupy granulations having a tendency to assume a diphtheritic character. Here, and for many forms of syphilitic ulceration, it can scarcely be too strongly recommended. At the same time, as far as regards lupus, I must not conceal from you that it sometimes leaves us in the lurch, while in other cases it acts extremely slowly.

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* See his article on Lupus, in Rust's theoret. prakt. Handbuch der Chirurgie, vol. xi. 1834.

As I have already told you, I employ all these remedies, with which I have formerly made many excellent cures, either no longer at all, or as adjuvants only, since I hit upon a method which has given still better results, both in respect to rapidity and certainty. It has, I believe, only one drawback, viz. that of being so painful as to render the production of anæsthesia necessary in all cases. In a surgical ward, in which chloroform is daily administered to several patients, this is of no consequence; in private practice, however, it may appear to many to be a circumstance which increases unnecessarily the difficulty of the treatment. But I must tell you that, for deep cauterisations, I always give chloroform to my patients, to spare them the pain which is here, in truth, not slight.

For the removal of parts which have been entirely converted into soft lupoid tissue, I use sharp scoops to scratch them off with; for the removal of diffuse lupoid infiltrations, with preservation of the layers of the skin, multiple, punctiform scarification.

The scraping off or scraping out with sharp scoops does not differ essentially in its effects from the action of caustics, caustic potash, or nitrate of silver applied energetically, but the employment of it is more convenient, quicker, and more easily measured. It produces such circumscribed and soft cicatrices as do not follow any other mode of treatment. Its especial usefulness for the removal of lupoid growths from the cavity of the nose or gums is self-evident. We cannot compare it with excision, because in the case of the latter we are obliged, on the one hand, to remove very much more, *i.e.* to make our incisions in the sound parts around the diseased; on the other hand, to form wounds with terraced edges, which afterwards furnish very firm cicatrices, while here flat, dish-like wounds are made, which become merged very gradually into the surrounding tissues, and the edges of the eroded parts (alæ nasi, etc.) are finely rounded off. As you will soon have an opportunity of seeing, the parts completely destroyed by lupus are always so softened that they may be entirely scraped off with the scoop, while the latter will never remove sound or even partly sound parts. You may employ considerable force without any fear of doing this. In the case of lupoid ulcerations of longer standing, an almost fibroid tissue becomes exposed after the diseased parts have been scraped off; which, indeed, never attains the extension observed in scrofulous ulcers, which, so to speak, often present an appearance resembling hailstones, but is, like these, to be regarded as the expression of reaction in the neighbourhood. But the lupoid exuberant growth is never so soft, loosely œdematous, as the granulations on the floor of scrofulous ulcers accompanied by formation of fistula and separation of the skin, out of which we can generally easily wipe these masses so unfitted for cicatrisation. On the other hand, it is always very much softer than the tissue found in ulcerating gummy tumours, which form the basis of the cutaneous tumours in so-called tertiary syphilis. The latter thoroughly resists the sharp scoop, and is, therefore, not amenable to the mode of treatment of which we are now speaking. Whenever, as is generally the case, a local treatment is here necessary, it must be carried out energetically with caustics. The difference in the physical condition of lupoid and gummy new-growths is so great that I should not hesitate, in doubtful cases, to use it with great confidence as a means of diagnosis, and perhaps the distinction between scrofulosis and lupus may, in a few cases, be made by its aid.

The sharp scoops which I use are partly the round one described by Bruns for scraping out carious bones, partly a narrower, long-oval one, especially applicable to the nasal cavity.

When the cleansing has been carefully completed, we may touch the wounds lightly with the stick of nitrate of silver; they are then to be covered with small pieces of lint, which is allowed to dry, or cold applications may be made.

Still much more, in comparison with the earlier remedies, is to be effected with the multiple, punctiform scarification. It has the advantage of attacking directly the diffused lupoid infiltrations without destroying the skin, and that in a very energetic manner, with speedy results. It is to be employed everywhere when the parts are not ulcerated, but diffusely lupoid, or still abnormally swollen and vascular; as well, therefore, in forms not connected with ulceration from the beginning as for the removal of the bluish-red spots remaining on the scraped parts, or in their neighbourhood, which incline so readily to returns of lupoid eruptions.

Hundreds, or even thousands of punctures, two or more lines in depth, are made close to each other with a very sharp-pointed, narrow-bladed knife in the affected portion of the skin. In many cases the skin, after the completion of the puncturing, appears of a bad colour, or even suspiciously white, and resembles chopped flesh; but gangrene has never ensued here. The punctured parts are then covered with lint, which is to be pressed on firmly to stop the bleeding, and then left until it falls off spontaneously. This operation is to be repeated at intervals of from a fortnight to a month for three, five, or even eight times. The first time, the knife enters very easily into the tissues, occupied largely by exuberant cellular growths, and the bleeding is considerable. Later on, there is more and more resistance, the skin gradually becomes firmer, and loses its abnormal swelling and redness. No cicatrices are formed.

As yet, no case of lupus has withstood this treatment, and the cure has, in the majority of cases, been very rapid. It evidently acts as well by the isolation of the numerous, newly-formed vessels as by the direct traumatic irritation of the tissues in a state of lupoid degeneration. A number of severe cases, existing for years, have been cured in from six to eight weeks. In about the same length of time we effected the cure of a girl 12 years old, suffering for five years from constant diffused lupoid infiltration of the nose, upper lip, and both cheeks, for whom all imaginable remedies had been used in the hospital itself without success. As regards the seborrhagic form alone, we are, as yet, without sufficient experience.

With such happy results, Gentlemen, you will think it natural that I should have become bolder from day to day in reference to the plastic restitution of portions of the face destroyed by lupus. I have already told you that I feel no hesitation about completing the treatment by means of a plastic operation immediately after the cure of the lupoid ulcers, without taking into consideration unavoidable relapses, contrary to the advice of many surgeons, who in such cases propose waiting some years to see whether the disease is completely eradicated. I do not even fear, in cases of necessity, to transplant lupoid portions of skin, and to attack the lupus afterwards in its new stations. This has sometimes great advantages, especially when the whole face, including the forehead, is affected at the same time, and the destroyed nose requires to be restored.

In these cases, the rhinoplastic operation was either not performed at all, or recourse was had to the skin of the arm for the formation of the new nose, a method to be avoided as much as possible, as well on account of its great inconveniences for the patient as of its imperfect results. The rhinoplastic operation from the forehead is, indeed, in these very bad cases, certainly, for the most part, impossible, if we first wait until the lupus is cured. The cicatrised skin, poor in blood-vessels, which is left, is no longer fitted for transplantation. On the other hand, the swollen and inflamed cutis, over-supplied with blood-vessels in florid lupus, can very well be used for this purpose, if we can only be sure of curing the lupus quickly afterwards. Lupoid tissues not degenerated to complete softening are greatly disposed to heal by the first intention, and leave very fine, scarcely visible cicatrices behind them; just as, for instance, in the exarticulation or amputation of fingers for extensive paronychia, where we are forced to take the flaps to cover the stump from the greatly infiltrated bluish-red skin, we get union by the first intention much more easily than in primary operations in traumatic cases, where the skin is perfectly sound.

Some months ago, I ventured, for the first time, in a case known to you all—that of a woman suffering from severe lupus extending over the whole face—to cut, for the formation of an entire new nose, a strip from the middle of the thoroughly lupoid diseased forehead, which was of a dark bluish-red colour, uneven, and studded with isolated, superficial ulcers; and we have cause to be very well satisfied with the result. The incisions both for removing the strip from the forehead and for paring the edges of the old nose, were everywhere made in tissues infiltrated with lupoid matter; yet the newly-formed nose healed everywhere by the first intention, and it soon became evident that the considerable anæmia which necessarily followed the severance of most of the vessels in the newly-transplanted slip, exercised a very beneficial influence over the morbid process going on in it for many years. The swelling went down rapidly, and the part became pale; the lupoid infiltration decreased visibly. After repeated puncturings, it now already presents almost entirely normal skin, while in the other parts of the face the lupus is very nearly cured. There can no longer be any doubt that we shall very soon be able to discharge this patient under the most favourable circumstances.*

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* The patient is since then completely cured.

ON INFANTILE PARALYSIS AND PARALYTIC CONTRACTIONS.*

BY

RICHARD VOLKMANN.

Gentlemen,—I show you to-day two cases of paralytic contraction of the foot, the history of whose origin is similar. In the first case we have a paralytic pes equino-varus; the foot touches the ground only with the outer border, and the heel is much raised. In the second case, the condition is, so far, the opposite of this, as the foot is in the position of abnormal pronation, and the boy supports himself entirely on the internal border of the foot, to such a degree, indeed, that the maleolus externus is placed against the external surface of the calcaneus. Besides, as you observe, the heel, in walking, is directed entirely forward, so that the projection of the heel totally disappears. The patient touches the floor with a part of the heel which lies very near the insertion of the tendo Achilles, as we see persons do after Pirogoff's amputation of the foot. You can easily draw back the heel, and give it a nearly normal position; but a further deformity becomes then all the more prominent; the sole of the foot is unusually hollow. This is, therefore, the so-called heel-foot, pes calcaneus, or, taking into consideration the abnormal pronation—the bending round of the foot outwards—a pes calcaneo-valgus.

In other respects the deformities are similar in these two children. In both, the affected leg is much emaciated, and the emaciation even extends to the thigh, although here it is less in degree; and in both, as you will afterwards see more fully, a number of muscles are paralysed, some completely, some incompletely. On the other hand, sensation is not disturbed. In both children also, the affected limb has been retarded in its growth. In the one the affected limb is 2 centimetres, and in the other 1½, shorter than the sound one.

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* Delivered in the Surgical Clinique at Halle, November 1869.

Both children presented nothing abnormal at birth. The little girl, who is at present 4 years old, was able to walk at 11 months, and up to that time had developed excellently. She was, however, as it appears, for some days rather unwell, did not eat and drink properly, and her head was somewhat hot, when suddenly one morning complete paralysis of both the right arm and leg was observed. The parts hung down loose and motionless, while the child otherwise felt again quite well. The paralysis of the arm disappeared in a fortnight or three weeks, leaving no trace, so that you can now detect no indication of it. The paralysis of the foot improved somewhat; it remained, however, to a considerable extent stationary, and led gradually to the formation of a club-foot of moderate degree, as you see.

In the second case, a boy, now 14 years of age, the affection which led up to the paralysis was of a much more serious nature. He was almost 2 years of age, and could walk very well, when he was suddenly seized with very violent eclamptic fits. When they subsided, there remained the paralysis of the right foot, which was also at first complete, so that the child could not, while in the sitting posture, raise or straighten out the dependent lower leg to the slightest extent. This last affection, however, which is referrible to a paralysis of the quadriceps extensor, also disappeared quickly, so that now there is only a certain weakness of this group of muscles detectable, while the majority of the muscles of the lower leg and foot remain completely paralysed.

This remarkable disease, which so suddenly deprives an extremity of its function more or less completely, is only observed in little children; and in the absence of any positive information as to its nature, it has been variously designated *infantile paralysis*, or *essential paralysis*, and also *spinal paralysis*. The last mentioned name, however, anticipates our present knowledge. There is much to be said for the idea that the seat of the permanent lesion is to be sought for in the spinal cord; and I shall have to mention, further on, some points in favour of this view; but still we have no positive facts to go upon. I myself believe, indeed, that at the first invasion of the disease the lesion is more diffuse, that the brain itself or its membranes are also involved. The nature of the process which does its work so rapidly remains perfectly unknown. It is particularly remarkable that all trace of irritation disappears so rapidly, or even does not come under observation at all. The disease is, at any rate, a sufficiently frequent one. I have myself already seen many more than a hundred such cases.

I have already mentioned that it is nearly always young children who are affected with this form of paralysis. The age of the little patients is nearly always between the seventh month and the end of the second year. Many of them are seriously ill at the time when the paralysis occurs, but generally for a few days only. Sometimes there are distinct evidences of hyperæmia of the brain, or, if you prefer to express it so, even slight appearances of meningitis; sometimes, as you have already heard, there are more or less violent eclamptic attacks. Other children show only an indefinite feeling of being unwell, are feverish for a day or two, symptoms which frequently occur at this age, without any evil result following, and without our being able to assign a cause for them; but in the present case the paralysis is at once detected. According to my experience this latter is the commonest course. But in still other cases, the children awake in the morning with the paralysis complete, after they have lain down at night perfectly well; or the parents can tell nothing about when the paralysis has occurred.

In the majority of cases there is only one limb affected. This condition I will name, for the sake of brevity, *monoplegia;* much most frequently is it the foot, and, in my experience at least, generally the right; more rarely, a single arm. In other cases it is both feet, *paraplegia;* or the hand and foot of the same side, *hemiplegia;* or occasionally even three, or, in very rare cases, the whole four extremities.

It has been asserted, indeed, that the hemiplectic form never occurs, or that the hemiplegia of children does not fall under the domain of infantile paralysis. And in this supposed fact some have looked for special evidence of the spinal nature of the affection.

But this is, according to my experience, altogether wrong. I have seen a large number of cases of hemiplegia which in every respect, and even in their reaction to the electric current, conducted themselves exactly like the monoplectic and paraplectic forms. And in particular, the affection not at all infrequently occurs at first as a hemiplegia, and one of the limbs, the foot, or more frequently the arm, recovers rapidly and perfectly, and only a monoplegia remains. This was the course of the disease in one of the cases you saw to-day, and in that case all symptoms on the side of the central nervous system were absent. I examined an exactly similar case a few days ago, in a girl aged 14. Here was a patient who was so little ill during the development of the paralysis, that her mother saw nothing wrong with the child till she suddenly discovered the all but complete hemiplegia of the right side. The further course was in this case so far a peculiar one, inasmuch as the foot recovered almost perfectly, while the arm remained paralysed. The parents, up to the time of my seeing the case, thought that the leg presented no perceptible alteration except a slight weakness. Yet there was a shortening to the extent of 1½ centimetres, a very distinct spareness and reduction in temperature of the entire right leg, and even, on external inspection, the pelvis was unsymmetrical. This partial restitution is specially characteristic of infantile paralysis. But there is in this respect the greatest irregularity, so that we can never say beforehand where the paralysis will afterwards be limited, and how it will be localised. I have seen a case of complete paraplegia in which, after two months, all the muscles were again able to contract, although slowly, except the tibialis anticus of one side, which remained permanently paralysed. Even in the less common cases in which the whole four extremities are paralysed, and the head falls back loosely, there is never such a general paralysis left permanently, many a time only an imperfect paralysis of one limb remaining. I have notes of two such cases, which, however, are distinguished by peculiar difficulties. In one of them, both lower extremities remained paralysed; a double talipes valgus developed, and both knees were strongly bent backwards, while in the superior extremities the entire muscles, except those of the right upper arm, became again capable of action. The young man was almost full-grown when I examined him in order to get a supporting apparatus prepared. An uncommonly muscular fore-arm was attached to a short humerus dried up like a stick. In the second case there remained a very moderate paraplegia.

But we not only observe that the paralysis often involves several parts at first, from which it afterwards disappears, but even where it attacks a single limb, the affection is generally at first very extensive, while, ultimately, single muscles or groups of muscles recover their power completely, or nearly so.

The motor paralysis is, therefore, in the affected member at first complete, or at least very extensive, but limits itself afterwards, and this occurs very rapidly. In a few weeks it is almost always determined what will remain paralysed or not; the disease very soon becomes stationary. If you wish to make the limit as wide as possible, you may say that, after the course of half a year or a year, you can expect no essential change in the state of the paralysis.*

This course distinguishes infantile paralysis completely from all other forms of paralysis, and so does the irregularity in the state of the different muscles. There are cases in which the entire muscles of one extremity remained permanently and homogeneously paralysed. Contrariwise, it has been observed that sometimes the paralysis disappears entirely. Kennedy has described such cases under the name of temporary paralysis. Many authors, among whom I shall only mention Duchenne, father and son, wish to eliminate these from the category of infantile paralysis. I do not think that you will, after what I have said, be inclined to agree with them, for these cases just fill up the peculiar features in the picture of the disease. It is, indeed, very strange to assert that where the disease occurs at first in the form of a paraplegia, but one leg rapidly recovers, we have one affection, and that in the lighter cases, where an original monoplegia is followed by complete recovery, we have an entirely different one.

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* In most text-books it is stated that the paralysed muscles very soon undergo fatty metamorphosis; they have even in France attempted to introduce the name Atropheé graisseuse for this disease. Carried to this extent it is quite wrong. Fatty metamorphosis occurs in these cases not infrequently in the course of time, but it may be absent in the severest cases even after years. I have, with my former assistant, Dr. Steudener, cut out little pieces from the completely paralysed muscles in several cases of some years' standing, and, as it chanced, we have not once seen an unequivocal fatty metamorphosis. At most, the primitive fibres appeared as if finely dotted, sometimes the nuclei were increased. Generally, we only found that the interstitial fatty tissue was much increased, and a portion of the bundles distinctly narrower than normal (for example, bundles of 0·003, 0·006, 0·0075 mm. diameter). I must, therefore, distinctly oppose the statement of both the Duchennes, that the degree of the paralysis always expresses exactly the intensity of the fatty metamorphosis.

Sensation is almost never disturbed in infantile paralysis. You are generally convinced of that whenever you try the electric irritability of the muscles; little children generally scream very violently under electrolysis. Further, the bladder and rectum are never involved. The activity of the sexual organs also is not interfered with. I have seen several adults with the severest paraplegia, depending on infantile paralysis, who rejoiced in an entirely undiminished sexual desire. The mental development likewise is not hindered.

The therapeutics of infantile paralysis, if you set yourself the task of removing or improving the paralysis, is as good as hopeless. I have not had a single good result in this respect, if the condition had existed for half a year or a year. Little is to be gained with the much-belauded electricity, whether you use the induced or constant current. In very recent cases the movements of individual muscles or groups will be quickly established under the electric treatment, but, in my belief, only those which would soon have recovered spontaneously. Any muscle which does not react to electric irritation some weeks after the beginning of the disease, remains almost always paralysed for the rest of life, whatever be the therapeutic means used. The electric irritability of the muscles is lost here, as in traumatic paralysis, very rapidly. The difference in this respect from the cerebral forms, in which this irritability is retained, is so great that some have deduced therefrom the chief argument for the spinal nature of the disease.

Although this seems a very sad statement to make, yet there remains much to be done by therapeutic agencies. And here begins the surgical side of this affection, which forms the chief subject of my discourse to-day.

For there occur in these children very marked secondary alterations, such as we see in scarcely any other cases where the conduction from the central organs to the organs of motion is interrupted; and the first of these is an interference with the general nutrition of the limb. It shows itself commonly in a few weeks or months in the most distinct way. The part is cold, blue, and the pulse smaller; the skin takes on a withered appearance. The adipose tissue and muscles, especially the latter, emaciate, so that the bony processes at the joints become sharply prominent, and the natural roundness of the joints disappears; and very soon it appears that the bones do not keep pace with those of the other side in their growth. Fortunately, the shortening is not generally very considerable, so that it does not, in the adult, generally exceed half an inch to an inch and a quarter. There occur, however, exceptionally, much more considerable shortenings. Many circumstances indicate that this disturbance of the general nutrition and growth is not to be explained from the defective use of the paralysed part. You will see cases where, for example, in the foot only a few muscles are permanently paralysed, and where a very slight deformity occurs; the children halt very little, and are on their legs the whole day, and yet the extremity lingers far behind in its growth. Especially noteworthy are the cases in which, as in Kennedy's temporary paralysis, the conduction is restored perfectly, that is to say, in all the muscles; but yet the trophic disturbances occur, and persist through life. I have seen this four or five times, and consider that these cases are full of significance for the whole history of the disease. I beg your particular attention to one circumstance, namely, the great extension which the trophic disturbance shows in every case, as you see in the two cases under observation. Even in very limited and imperfect paralysis it always extends over the entire limb. You can commonly follow its last traces on to the trunk, the pelvic zone, the shoulder, even the head.*

The state of matters then is, that of the muscles paralysed at the beginning of the disease, a large portion become afterwards completely, or almost completely, capable of action; that, however, a weakness remains in the parts, as far as the paralysis originally extended. They never recover perfectly from the severe blow which has struck them.

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* A case of unilateral atrophy of the face appeared to me to fall quite evidently into the domain of infantile paralysis. The mother of the 9-year-old girl stated that within the first year of life the one arm had been for several weeks perfectly paralysed, and she referred this temporary paralysis to an attack of teething-convulsions. It was soon afterwards noticed that the face developed unsymmetrically.

In the second place, there occur, very soon, further changes which tend to interfere in a high degree with the usefulness of the limb whose function is already lost, or seriously impaired. The articulating surfaces of the joints change their form and their respective positions; the affected portions of the limb assume altogether wrong postures; in short, there occur the so-called paralytic contractions which present themselves, especially in the lower limb, in the various forms of club-foot. And while the paralysis itself, as we saw, soon becomes stationary, and the atrophy reaches its maximum after a moderately long time, we find that these latter changes show the greatest tendency to increase up to the time when the skeleton has reached its full size.

The great majority of cases of club-foot, and deformities of the feet which develope after birth, are paralytic in their nature, and referable to infantile paralysis.

The common form is the pes equinus, with slight, or, more rarely, considerable, varus-position. More rarely there occurs the flat-foot (valgus); still seldomer a pes calcaneus, or the complicated affection which we have in the boy before us, the calcaneo valgus. In the knee and hip-joint there arise certain alterations in the mechanism of the joints which we shall touch on afterwards; but even in the most severe cases there is no contraction. The upper extremities conduct themselves somewhat differently. The joints of the fingers and hand almost always assume permanently the flexed position. The elbow-joint remains free. In the shoulder the power is almost always, in severe cases, lost, of even passively performing an action which appears unnecessary to the paralysed arm, and is, as a matter of fact, not performed by it, namely, the complete elevation of the arm. The pectoralis, latissimus, etc., appear more or less contracted; at least this was the case in almost all the cases observed by me, which, as already mentioned, numbered over a hundred. We may say, then, that this is the general rule.

It is quite evident that the foot is, of all the parts, the worst off; the deformities to which it is subjected are by far the most severe and troublesome. Of course, a paralysed arm is hardly or not at all used, according to the degree of the paralysis, but a paralysed foot must be used. And we may premise that almost all those children with essential paralysis, in which one or both legs are affected, manage to do without crutches, though their locomotion is often imperfect and difficult enough. Only in the rarest and most severe cases is their use necessary.

What, then, is the cause of these paralytic contractions? How do they occur?

The theory which you will find in all the text-books is the so-called *antagonistic* theory. Delpech was the first to formulate it perfectly. It proceeds on the supposition that the paralysis, when it leads to deformity, is always partial, or, at least, is not homogeneous. The muscles which are not paralysed, or only slightly so, draw the limb to their side. The contraction which they experience is at first only functional; but through time they lose the power to relax. This latter condition was distinguished from contraction as retraction.

This theory is sufficiently simple. The only question is, what may be the stimulus which excites the unaffected, or only slightly paralysed muscles to this uninterrupted contraction which goes on to absolute nutritive rigidity. It has been thought that this stimulus is to be found in the so-called *tonus.* It was supposed that the muscles are normally in a continual state of slight contraction. A constant stimulus proceeds from the spinal cord, which produces in the apparently quiet muscles a slight contraction. Under ordinary circumstances the muscles mutually antagonise each other, and then results the position of the parts which they assume in the state of rest, just as the mast of a ship is kept in position by the tight cords on every side. If the cords are cut on one side, if the muscles are paralysed, the mast falls, the limb moves to the opposite side.

It is certain that such an arrangement exists in connection with the vascular system. But the existence of tonicity in the voluntary muscles is exceedingly doubtful. As surgeons, at least, we can leave the tonus completely out of view; we have not to inquire whether the tonus exists as an exceedingly weak force, but are rather concerned with definite and important actions. We have to ask whether the tonus of the unparalysed side is capable of overcoming the weight of a limb, or portion of a limb. This would be necessary in order to an explanation of the abnormal positions which occur. Now, Gentlemen, no such force can for a moment be thought of. Its existence is contradicted, among others, by the simple fact of the pendulum movement of the arm at the shoulder and elbow in walking. The slightest change in the centre of gravity produces at once movements of the joints. The slightly bent position of the elbow in the loosely-hanging arm is not due to the fact that the muscles from every side act continually on the joint, and that, as was at one time stated, the flexors preponderate over the extensors, but it depends on the elastic tension of the whole parts which form and surround the joint; it remains, therefore, even after death.

It is now twenty years since Werner entered the field with the keenest weapons against the muscular tonus, and its use to explain the paralytic contractions. He was not understood at the time, or the people did not wish to understand him, especially as he was modest enough to call himself the reformer of orthopædic surgery, and his polemic assumed a very aggressive character. But the theory which he sought to put in the place of the tonus, and by whose aid he sought still to preserve the antagonistic character of these contractions for the majority of the cases, although it contains many errors, still presents one point which is thoroughly correct, and this we shall now stop to consider.

He called attention to the apparently self-evident fact that a muscle is able to contract actively, but is not capable of actively relaxing. If, then, as he says, the extensor muscles of a limb are paralysed, but the flexors not, then whenever the patient seeks to move the limb, it will go into the position of flexion, and if now it does not fall back into the position of extension by its own weight, or is replaced by the hand or otherwise, then it will remain flexed. At any rate, it will happen that the limb is often and long in the state of flexion In this way permanent curvatures will develope.

Werner used this theory to explain the obliquity of the face which so rapidly follows on facial paralysis—a kind of case which, to this day, brings over most adherents to the tonus theory. The contraction of the muscles of the sound side of the face does not occur exactly at the moment of the paralysis, but appears only on the next occasion of the use of speech or the play of features. The muscles which have contracted remain so, because the antagonists are out of action, and the weight of the cheeks is not sufficient to cause their relaxation. It appears as if this were the true explanation. Following Werner's advice, I have, in two cases where the facial nerve was divided in large operations on the face, seized the drooping upper lip with the finger, and pulled it into its proper place. The asymmetry vanished as long as the patient could be prevailed upon to keep his face motionless.

But, even taking this new view into account, the antagonistic theory is not of great importance for the development of paralytic contractions, at least in the foot, which is of most interest for us. The contractions in this case by no means always develope according to the antagonistic scheme, as C. Hüter was the first to point out with certainty.

This, however, is just the point which we require at the outset to investigate, whether each individual case falls in with the theory. Whether, in fact, these deformities only arise where a portion of the muscles alone is paralysed, or, at least, the paralysis is not homogeneous; and whether actually the limb is always bent towards the side of the sound or less paralysed muscles.

This is not infrequently the case, but by no means always.

The following are the results of a large number of investigations specially directed to this point:

1. Even in complete paralysis of the entire muscles of the lower leg, we may have the severest forms of club-foot;* and in complete paralysis of the muscles of the fore-arm, the most intense contractions of the hand and fingers.

2. In imperfect but very extensive paralysis, it not infrequently happens that in the arm, as well as the foot, it is the muscles which lie in the concavity (therefore the shortened ones) which are specially paralysed, while, according to the antagonistic theory, it should be the opposite.

3. But even in paralysis of one group of muscles exclusively, the deviation may occur in the direction of the paralysed side.

I will lay before you afterwards special evidence for these statements. If they are trustworthy—and you will soon convince yourselves of this by the investigation of children so affected—then it follows that the deformities met with are, at least in many cases, not due to muscular force.

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* Linhardt had already observed this, but confined himself to the simple authentication of the fact.

Werner, whose name I have already mentioned, was on the highroad to the solution of this problem; yet he missed it.

The following case came under his notice. He had already performed tenotomy on one side in a case of congenital (therefore not paralytic) club-foot, the patient being a very intelligent boy, who stood the operation very quietly. He now proceeded to divide the tendo Achilles on the other side in the following way:—The boy sat on a table with the leg hanging down, and an assistant stretched the tendon, in the usual way, by dorsal flexion of the foot on the ankle. The tendon was now divided, and, at a given signal, the assistant let the foot go free. It fell back at once, by its own weight, into the most marked equinus position. Werner, if he had gone a step further, would have overturned the antagonistic theory. He only required to say to himself, If this had been a case in which the muscles of the calf were put out of action by paralysis, instead of by division of the tendon, then the patient might have let the foot fall in the same way; and if the extremity had not been used for months on account of the paralysis, then there might have been developed during this time a pes equinus, although the theory of antagonism necessitated the formation of a pes calcaneus.

It remained for C. Hüter to draw this conclusion, and we owe to this author a series of most praiseworthy investigations on the mechanism of the joints, and their disturbances. He showed that the mass of the foot is so unequally distributed around the axis of motion of the joints, that when left to assume any position by its own weight, the foot not only droops at the toes, or, as it is commonly said, stretches out (plantar flexion), but, at the same time, makes a second movement, by which the internal edge of the foot comes to stand higher than the outer (supination), and the great toe passes inwards (adduction). I must, unfortunately, confine myself to these passing indications. The experiment is so simple a one that you can at any moment test the correctness of the statement.

This position, which the foot takes of itself when left quite free from the action of the muscles, is exactly that which we observe, as a rule, in paralytic deformities, although in a higher degree. And you can very often convince yourselves that deformities of the foot may result from the influence of gravity alone; as in cases of compound fracture of the lower leg, which are not treated with stiff apparatus, which fix the foot, but with some kind of splint or bandage. In such cases, a very firm equinus position commonly occurs, with a combination of varus, if the sole of the foot has been insufficiently supported. There is only by degrees a return to the normal. You not infrequently see in these cases, after their cure, a hollowness of the sole, a higher degree of which is so often present in paralytic club-foot, and which is generally referred to contraction of the plantar fascia. It arises apparently from the point of the foot (toes and metatarsal bones) sinking gradually but considerably, in consequence of their weight, the shortening of muscles and soft parts being secondary (Hüter).

In cases of severe internal diseases, in which the patient is bedfast for months, there sometimes develope deformities of the foot of a similar type. A case of this kind occurred to me. A lady had a severe attack of typhoid fever, then a relapse, and after that lay in an exceedingly weak state for months, motionless in bed. When, at last, the first attempt was made to stand up, they were shocked to find that a club-foot had developed on both sides. There was no paralysis of a single muscle. The feet, however, remained so firmly in the wrong position, that it required a year's orthopædic treatment to enable the patient to walk.

We must, therefore, ascribe to these purely mechanical conditions an important part in the development of the ordinary forms of paralytic club-foot. Yet, in a number of cases, we see other, and even the opposite, forms of club-foot appearing. How are these to be explained without the active assistance of the muscles? And, further, how do the contractions occur in other joints?

In order to decide these questions for the lower extremity, I took a somewhat different course from Hüter. I allowed the children to walk, and studied their gait. I had abundant opportunities for this in the various stages of the disease. To my own astonishment, I was at first much assisted by the relations of the knee-joint. I had long ago observed that no proper contraction ever takes place at the knee, even when the paralysis extends to the thigh, that, on the contrary, the joint becomes too moveable, and this always in the same way; it is over-extended. The thigh and leg come to form an obtuse angle, open anteriorly (genu recurvatum). This deformity does not usually reach a very high degree, but even that occurs. In very advanced paralysis, the joint may become quite loose. In the hip-joint the same kind of thing occurs. An abnormal looseness of the capsule always shows itself. We can produce movements on the paralysed side which are quite impossible on the sound one. In our two patients, for instance, I can, without causing pain, pull the thigh outwards till the point of the foot almost looks backwards. This symptom is so seldom absent, even in cases where the paralysis is confined to a few muscles of the thigh, that in slight cases it may be used for the purpose of rapidly confirming the diagnosis.

My surprise increased when I convinced myself that the genu recurvatum was always associated with a weakness, or imperfect paralysis of the quadriceps femoris; that, in fact, it even developes in cases of perfect paralysis of this muscular group. And the flexors may have retained their full power, or nearly so. Here is, therefore, one of the cases in which the deviation occurs in the direction of the paralysed muscles.

It is easy to understand how no permanent contraction occurs towards the convex side, no permanent flexion. The extremity falls of its own weight, both in the upright and recumbent posture, into the position of extension, and the flexors of the knee are often enough stretched, even in perfect paralysis of their antagonists, to prevent them, as it were, rusting in their position of contraction.* But how does the over stretching of these muscles come to pass?

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* In saying this, I refer to the great majority of the cases, but, in order to prevent misconception, I ought to add, that contractions may develope under exceptional circumstances, both at the knee and hip, first of all, where crutches are used. In such cases, the patient requires, during loco-

In this way: the patients endeavour to walk with as little assistance as possible from the quadriceps. They walk exactly like a patient who has had his leg amputated in the thigh, and who wears an artificial leg. The mechanism of such artificial legs is commonly a very simple one.

The stump is set in a socket which rests on the buttocks and ischial tuberosity. A leg, with foot, is attached to the socket, with a knee moveable by means of a hinge. This hinge is so arranged as to allow the knee to be bent, but not to be extended beyond 180°. When the full extension is reached, a check comes into action, just as in the natural knee. You have an arrangement such as exists in every pocket-knife.

In order to understand how the patient walks with this simple apparatus, how he bends and straightens this knee-joint, devoid of all muscular power, and under what conditions he is able to rest the entire weight of the body on the artificial foot, you may take in your hand a pocket-knife, resting it by its point on the table, and with its back turned away from you. The blade corresponds to the lower leg, the joint to the knee, and the handle to the thigh; your hand, which is supported on the handle, corresponds to the body of the patient. You are able, as you see, by slight variations in the direction of the pressure, to cause the blade to move at the joint. Everything depends on the relation of the weight, which is represented by the pressure of the hand, to the pin of the joint, around which the blade moves. If the weight falls behind it, that is to say, on the side of the edge of the blade, then the knife closes, if you press strongly enough; if it falls in front, it opens; and if it is fully opened, then you can press as heavily as you choose on the handle.

Just so is it with the patient and his artificial leg. He moves the knee-joint by letting the weight of his body fall, sometimes behind, sometimes in front of the axis of motion (the hinge). He must learn to do this, but he learns it rapidly. If

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motion, to keep the paralysed extremities drawn up towards the body, in order to prevent the foot striking against the ground. It also occurs in totally neglected children, who lie for years curled up on their sides in bed. These are, however, exceptional.

he wishes the weight of the body to rest on the artificial foot, he must always let the centre of gravity fall in front of the joint. If he overlooks this, then the limb collapses under him, and he is thrown on the ground, an accident which occasionally happens to these people. The use of the leg, however, is essentially facilitated by the instrument-maker, by placing the hinge of the knee as far back as possible, and by making the check a little over 180°. He gives the leg a slight degree of genu recurvatum, in fact.

I have seen several cases in which the patients, in consequence of infantile paralysis, had lost the power of all the muscles which move the knee, yet they could walk, and that without crutches. I have more frequently seen cases where only the extensors were completely paralysed, while the flexors acted strongly; and a still larger number, where a more or less considerable weakness of the extensors could be detected; and in all these the mechanism of walking was the same. They brought the paralysed foot forwards—and this, in the most severe cases, could only be done by a kind of swinging motion—and then let the weight of the body act on the knee in such a manner as to bring it into a position of complete extension, and keep it there. In this way, the leg could not collapse, either forwards or backwards. In front, the weight of the body pressed the articular surfaces of the femur and tibia firmly against each other; behind, the ligaments prevented the gaping of the joint. The ligaments and bones require to support the whole weight of the body. It follows that, through time, this physiological check apparatus must deteriorate; the nutrition of the individual parts is interfered with in consequence of the general atrophy; in fact, the material of which the mechanical apparatus is constructed is, so to speak, inefficient The ligaments in the bend of the knee relax, the overladen bones remain, in their growth, somewhat too low in front; the knee is over-stretched; a genu recurvatum is formed. If it does not reach a high degree, as is commonly the case, then it assists, in bad cases, the walking more than it hinders it.

Exactly similar relations are repeated in the hip-joint. You have already convinced yourselves of the great looseness of the ligaments which can be made out in it. This not infrequently leads to an over-extension, by a gradual stretching of the anterior part of the capsule. Although the strongest ligament of the human body, the ligament of Bertini, is woven in with it, yet the ligament gradually gives way when the patient continually allows the whole weight of the body to act on it. This happens when, instead of supporting the pelvis, he allows it to fall backwards as far as this ligament allows. It appears, then, as if the neighbourhood of the symphysis pubis were strongly pushed forwards, and the anterior convexity of the lumbar vertebræ increased. In truth, young children so affected assume a position somewhat similar to that met with in congenital luxation of the hip-joint. I have seen several errors in diagnosis brought about in this way; even the well-known Verneuil allowed himself to be so far deceived as to assert that what we commonly call congenital luxation of the hip, is not generally a congenital state, but a secondary dislocation, in consequence of infantile paralysis.

The cause, therefore, of a number of these paralytic deformities of the joints is to be sought for in the fact that the patients are not able, by means of the muscles, to equilibrise the individual moveable parts of the skeleton, or, at least, are able to do so imperfectly. In consequence of this they push the motion of the joints to the utmost limits at which the physiological check apparatus comes into action, in order that they may avail themselves of the weight of the body to effect their object. The limb is then used as a rigid support.

We have here to do with conditions that have been long associated, to a greater or less extent, with the causation of another class of deformities, which, indeed, appear at first sight to have little in common with the paralytic contractions, but yet occur, like these, only in young persons in whom the bones have not yet completed their growth; I refer to the so-called habitual scolioses, the common flat-foot, and the genu valgum. These also occur only under circumstances where the bones and ligaments are charged with work which should fall on the muscles, whether these latter are absolutely too weak, or only so in relation to the action required of them. Even laziness plays some part in their production. It is not sufficient to say that the difference between these everyday forms and the paralytic contractions is only one of degree. We have to notice that in both cases we have the same mechanical conditions; for in the cases of infantile paralysis we have often no proper paralysis at all, but only great weakness, and a defective state of nutrition; and yet the deformities occur.

Now, Gentlemen, if you will cast your eyes back on what has been already said, you will observe that there are three mechanical conditions which have to do with the development of the paralytic deformities: the weight of the affected part itself; the abnormal burden laid upon it when in use; and, lastly, the ultimate inability to recover a limb from a position into which it has been brought by unparalysed muscles. Of these, the least influential is the last, because even in the absence of antagonistic muscles the limb generally falls back into its original position by its own weight. Every individual case can easily be explained by a proper combination of these three conditions.

The most common deformity which occurs in the foot, is, as already stated, the pes equino-varus. You have already seen that this results from the fact that the foot, left to its own weight, falls of itself into this position. The standing posture, by lading the foot with the weight of the body, acts against the production of this position. Therefore, the younger the child when the paralysis occurs, and the longer it is in learning to walk, the more easily does the foot fix itself in the abnormal position. In this relation we may call to mind the circumstance, which Dieffenbach was the first to point out, and which afterwards Hüter confirmed by special observations, that newly-born children, and those who have not yet learnt to walk, present, in relation to the form and position of the joints of the foot, very different relations from adults. So great is the difference, that Dieffenbach was justified in making the paradoxical statement, "All children are born with club-foot of the first degree." Only by afterwards walking does the foot step, so to say, into the form of the adult's. If the child, on account of the paralysis, uses the foot timidly; if it is not able to let the full weight of the body rest on it at intervals; then the foot preserves more or less that form which, when fixed, is already a club-foot. If, again, the growth of the whole leg is delayed, then the formation of a pes equinus is further promoted. In this case, even if the muscles on the anterior surface of the lower leg are unparalysed, the child will naturally let the toes droop, in order to reach the floor. I can show you an example of this in the girl with the paralytic pes equino-varus. The perinæal muscles, and the extensor digitorum, react very distinctly on testing them with electricity, although they are but weakly developed in connection with the general atrophy of the leg. The muscles of the calf, the tibialis anticus, and the flexor digitorum, do not react in the least.

If the child, on the other hand, is older when the paralysis occurs; if it can already walk, and soon takes again to walking, then there occurs very readily a paralytic flat-foot. It is of no consequence in this respect whether it is chiefly the tibiales or peronæi which are paralysed. I have particularly noticed that in cases of severe paraplegia a double pes valgus always developed, provided the patient walked without crutches. In these cases the patient walks on the full sole, and the weight of the body, which models the foot of the newly-born into that of the adult, does not meet with any resistance from the paralysed muscles. On the contrary, the patient, exactly as in the case of the knee and hip, purposely allows the foot to curve outwards till the physiological check is reached, and the parts become fixed by it. It is not long till the abnormally weighted ligaments stretch, and the bones on the compressed side are interfered with in their growth. A pes valgus thus forms. It forms in exactly the same way as it does in a baker's or blacksmith's apprentice, who requires to stand on his feet till the muscles are completely exhausted, or who has the joints of his feet too heavily weighted by carrying too heavy sacks, or working with heavy hammers.

But the foot of the paralytic is in so far in a better position than that of the apprentice, he saves it much more. The patient who has been afflicted with this infirmity from his childhood chooses an occupation which allows of a more sedentary way of living, and if he is at any moment not using the foot, he leaves it, just because of the paralysis, to the passive effects of its own weight much more uniformly than the apprentice will ever do. But gravitation acts directly against the flat-foot position—rather seeks to bring the foot into the position of a slight club-foot (equino-varus). The muscles, whose insertions are abnormally approached while the foot is pressed into the valgus position in walking, are again drawn out in the state of rest. Contractions, therefore, form in them, for the most part slowly, and in a less degree; the paralytic club-foot soon becomes fixed; the paralytic flat-foot remains long moveable, not infrequently in such a way as that the foot actually hangs loose.

This relation shows itself still more in the heel-foot (calcaneus), of which we have seen such a characteristic example to-day, albeit in combination with a simultaneous valgus position. While in the case of the little girl, who is only 3 years of age, it requires a large amount of force in order to bring the foot back to its normal position, it is sufficient, as you see, in the case of the boy who has already been affected for twelve years, and has continually walked on the deformed foot, to bring the lower leg into the horizontal position, in order to allow the heel, which projects forwards, to fall back into a nearly normal position. Even the coexisting valgus position, which is here of quite unusual extent, almost disappears when I hold the foot so that its outer border looks upward. What remains of the deformity, I can cause to disappear with a slight pressure of the hand. The peronæi are somewhat contracted, but quite extensible. Only the one element remains which this deformity has, in common with the pes varus, namely, the concavity of the sole. It is impossible to remove it, even by using the greatest force.

I have in past years had under observation five cases of pes calcaneus in children of 5 to 15 years. They all showed exactly similar relations. In all the five cases the muscles of the calf were completely or partially paralysed. Yet the pes calcaneus cannot in the least degree be explained on the antagonistic theory. Apart from the fact that it is impossible to imagine what the muscles may be which could pull the calcaneus in this way forward, there are two other considerations which render the antagonistic theory impossible. In the first place, you cannot conceive that the muscles are able to draw the calcaneus forward against the action of gravity, and to such a degree as to alter completely the shape of the bone; and, in the second place, you have to consider that as soon as you leave the anterior part of the foot to its own weight, the calcaneus at once falls back nearly to its normal position.

The mechanical history of the pes calcaneus is exactly the same as that of the genu recurvatum. Let us ask our patient to walk. You see that so long as the foot is suspended in the air during walking, the deformity is slight. But now, when he sets his foot on the ground, the more he places it in front of him the more does he touch the floor with a part of the heel which does not belong to the natural surface which comes in contact with the ground, but is situated above it towards the insertion of the tendo Achilles. At the moment when he uses the paralysed foot as a support, and swings forward the body on to it, then the calcaneus juts forwards as far as the ligaments, already stretched by this mode of progression and the altered bones, allow, the paralysed muscles of the calf being unable to keep it in position. The concavity of the sole, which reaches a higher degree in this deformity than any of the others, is produced in part by the drooping of the point of the foot by its own weight, and in part by the fact, that on account of the forward displacement of the calcaneus, the insertions of the muscles of the sole, and of the plantar fascia, are continually approached.

The pes calcaneus is rare, and this cannot surprise us. The commonest form is, as you know, the pes equino-varus, in which the heel in walking no longer reaches the ground; or, again, the child takes very short steps, so that the full sole is placed on the ground, in which case a flat-foot developes.

The relations are much simpler and more uniform in the paralysed *arm.* Although the same rule applies to it as to the lower limb, that the most intensely paralysed muscles are the peripheral ones, and that the paralysis and atrophy gradually diminish towards the trunk, yet the most serious changes occur in the hand and shoulder-joint, while the intermediate *elbow-joint* remains almost unaffected.

This, of course, does not correspond with the earlier views, but it cannot surprise us. The paralysed arm hangs down with the elbow extended; in walking, it wags backwards and forwards; in sitting, it is rested, for the most part, in the bent position on the patient's lap. Under these circumstances, how can a contraction occur? You will be convinced, however, that it may occur, if you meet with a patient who has during childhood worn his arm in a sling for a long time on account of the paralysis.

At the same time, you can nearly always detect one alteration in the elbow-joint: the ability to supinate the fore-arm is diminished, or quite lost. And why? Because the patient, if once the paralysis is such that he cannot use his hand, loses the habit of supination. The back of the hand of the drooping, inactive arm is directed forward and outward, and this corresponds with moderate pronation.

In the *shoulder-joint*, also, the patient has no call to make any extensive movements. The upper arm lies more or less closely by the side, and its elevation, especially, is not exercised. The muscles which are passively stretched by this movement, the pectoralis major, latissimus, etc., do not consequently develope to their proper length; or, if you prefer the expression, they show a greater or less degree of contraction. We do not usually find a paralysis of the deltoid which elevates the arm. Where it exists it will still further promote a contraction of the shoulder, but not on antagonistic grounds.

If, however, the muscles which proceed from the scapula, thorax, and clavicle, to the arm, and support it, are to a considerable extent weakened or paralysed; then the weight of the arm acts especially on the shoulder-joint, it hangs as a continual weight on the capsular ligament. The capsule gives, as in the other cases, and the head of the bone falls down; and this occurs sometimes to such a degree that we can dip the fingers in pretty deeply between it and the acromion. This has been often described as the luxation of atony, or relaxation.

The *hand* always shows the most distinct alterations. The fingers are flexed, drawn into the palm. The wrist is, at the same time, held in a slightly flexed position. The extension of these parts meets with more or less difficulty. If you flex the hand strongly, you can stretch the fingers passively, but it is then all the more difficult to extend the wrist. If you double up the fingers, then you can extend the wrist, at least to 180°, but not generally much further. The muscles whose tendons pass over both the wrist and finger-joints are, therefore, too short.

If you examine more fully the muscles in these cases, it often appears that the flexors are less paralysed than the extensors, or even that the latter alone are paralysed. In other cases both groups are equally paralysed. But cases occur where the extensors are less paralysed than the flexors, or even remain entirely unaffected, and yet the same contraction towards the side of flexion arises. I have lately seen two additional cases of this kind, and both were shown in the clinique. In one case the flexors did not react in the least, and the extensors reacted very promptly, though rather more weakly than normal. In the other case, the extensors, as well as the flexors, could still be moved spontaneously to some extent, but the extensors were very distinctly stronger in their contractions than the flexors. I must not forget to say, however, that only one was a case of infantile paralysis. In the other, the paralysis resulted from a bandage applied too tightly in fracture of the fore-arm.

There is no question that the contraction of the fingers in paralysis of the muscles of the fore-arm is produced by the position which the fingers assume when left to themselves. The fingers are not heavy enough to fall passively into the extended position when the arm hangs down. They remain in a slightly flexed position, which is retained unaltered even during the pendulum-motion which the shoulder and elbow-joints undergo in walking. This is their so-called intermediate position. It arises from the elastic tension of the ligaments and muscles, the position of the articulating surfaces, etc., and, therefore, remains unaltered in the dead body, even when it is beginning to decompose. The contraction develops out of it if the hand is not used, the fingers not extended, and the flexors, therefore, not often enough, or not at all, passively stretched.

Gentlemen, we have not yet been fortunate enough to discover any means whereby the interrupted communication between the nerve centres and the apparatus of locomotion in infantile paralysis can be re-established. It is very unlikely that any such means will ever be discovered. All the more necessary is it, that the physician should, from the very first, direct his entire attention to those secondary changes which develope in the paralysed limb, in order to preserve the member in a state, at least, of comparative usefulness.

First of all, the child must be made to walk, and that as soon as possible, and he must be made to place the entire sole on the ground.

The atrophy of the limb does not, certainly, depend altogether on its defective exercise, but it is essentially increased by that; and, since the atrophy always extends much further than the paralysis, and there are nearly always a great number of muscles besides the paralysed ones, which are kept back in their development and weakened, we can, by properly directed use and methodical exercise, attain some essential advantages. Even slightly irritant embrocations, baths, and such-like, are to be recommended; all the more, as the parents generally lay some weight on such means. We only meet with considerable shortening of the bones of the lower limb where the leg has been very little, or not at all, used as a support, and especially where the patient has used crutches. But it is almost always possible, even in the most severe cases, by the aid of splints and articulated supporting apparatus, to make these patients walk without crutches. The use of crutches must here, as in deformities which occur after healing of inflammation of the joints, be prevented in children in every way possible. In the worst cases, you should let the patient use, instead of crutches, a little stool, whose seat is represented by a small, rounded ledge, from which the four legs proceed, these being about the length of the entire lower limb. The patient supports himself on this little stool with the hands, and helps himself on with it by lifting or pushing it before him.

If the child walks, its gait is to be carefully watched. The development of every one of these deformities may be easily prevented by the most simple means, if attended to from the first. This is very particularly the province of the family attendant, who, unfortunately, too often leaves this matter, in which he can accomplish something essential, while he uses all kinds of internal means, in order to act on the paralysis. A lacing-boot with a steel spring, which acts lightly, sometimes on the inside, sometimes on the outside of it, or with the sole raised on the outside or inside, is sufficient, at the beginning of the affection, to prevent the development of a varus or valgus. But we must take particular care to prevent the formation of an equinus, or strong concavity of the sole. In order to this, it is perfectly sufficient to fix the foot every night on a board padded with cotton wadding, by means of a flannel bandage, and to draw up the foot by a strip of adhesive plaster towards the lower leg. The operation is so simple, that every mother learns it immediately, if she is told the object for which it is done.

If a pes calcaneus begins to be formed, then the paralysed muscles of the calf can be easily replaced by an indiarubber spring, which is fixed below at a point in the heel of the shoe corresponding to the insertion of the tendo Achilles, by means of an eyelet, and above, under the knee, by a hoop of padded iron plate. This hoop is supported by a rod which passes down the inner side of the foot to the sole, and is let into the latter in the usual way. Most of these indiarubber springs have, however, much too little resistance, and are not arranged properly. The simplest is a ring, about the size of a thaler (about a crown-piece in size), made of indiarubber of about the thickness of the finger; these are kept in the shops for technical purposes. Two leather straps with buckles are carried through this ring and fixed, the one, by means of a hook, to the eyelet, and the other to the hoop. The strap can be made longer or shorter by shifting the buckle, and so the patient can regulate the tension of the indiarubber. The apparatus is at once very cheap and very efficient. Even in the worst cases of pes calcaneus, the patient may find advantage from its use.

You may suppose that where we have paralysis of certain other single groups of muscles, these may be replaced by indiarubber muscles; but actual experiment does not come up to the expectations which we might *à priori* form. If the shoe is not to be completely shapeless, then the indiarubber springs must pass at the bend of the foot over rollers. The arrangements are thereby made too complicated, and, besides, much force is lost.

In order to improve the older and more severe deformities, we must use forcible methods of correcting the position under chloroform, or by plaster of Paris arrangements, and so on. Tenotony should be avoided as much as possible, because the already weakened muscles may very readily be still more interfered with in their function thereby. As a matter of fact, this operation can, in the majority of cases, be avoided, as even the paralytic pes equinus, or equino-varus, opposes much less force to its reduction than, perhaps, a congenital neglected club-foot.

ON REFLEX PARALYSIS.

BY

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GENTLEMEN,—The patient that I bring before you to-day, you have already seen a few weeks ago. Some important changes have taken place in his condition since then, and these we shall study more closely. You remember that before his admission to the hospital he had been suffering from dysentery, which still persisted in a moderate degree. A short time before his admission, there had supervened on the dysentery a painful affection of the inferior extremities, combined with pains in the back and hips, which radiated downwards as far as the joints of the foot, and were associated with distinct functional disturbances. In addition, there was hyperæsthesia, and increased reflex irritability of the inferior extremities, as well as a painful affection of the knee-joint, associated with swelling on the right side. This affection, which we then explained to be a neuritis lumbo-sacralis migrans, occurring as a consequence of the dysentery, has developed still further, and has even extended, as you will see, to the spinal cord.

The interest and importance of the observation requires that we should repeat the whole history of the case. We shall do so, following the reports in the journal.

R. Schmidt, a workman, aged 44, has not had the usual diseases of children, nor, till now, any serious illness. On the 12th of October last, the patient observed that he evacuated a mucous and bloody stool, after severe tenesmus, and, in consequence, he sought medical advice. He was ordered castor oil, and a white powder. After he had taken twelve powders the pain began to abate, and, after twenty-four, the blood disappeared from the stools. The patient estimates the amount of blood, mingled with mucus, which he lost during these fourteen days, as about two to three quarts. After the patient had been feeling quite well only for a few days, he experienced a stitch in the left lumbar region, which extended to the sacrum, and afterwards passed to the right thigh as a gnawing pain. The patient then applied to the medical outdoor dispensary [Poliklinik], the tenesmus still persisting, and again received castor oil and the powder, and was directed to lie in bed. Some days after, he experienced a pain in the right knee-joint, which daily increased in intensity; it finally involved the entire lower leg down to the maleolus, and prevented the patient walking, or even standing. The right knee swelled a little. As the pain in the lumbar region did not diminish, the patient applied twenty cupping-glasses; and as this had no effect, he put on a mustard plaster. As a result of this, the pain in the lumbar region disappeared, but persisted in the sacrum. For the last eight days pain has existed in the left leg, and especially in the knee, but without swelling.

This note was made on November 21st, 1869, the date of admission, and the following was the condition then:—The patient is well formed; muscles pretty strong; subcutaneous fat spare; face pale and expressive of suffering, but otherwise normal. He lies continually in bed, generally on his back, but is able to turn on his side. He complains of gnawing pains in both knees, extending down the tibiæ, but more severe on the right side than the left. These pains are continually present. He further complains of pain in the back, which occurs when he lies on his side; he is not able to walk, from the severity of the pain in the knees and tibiæ on standing up. The patient thinks that the weakness of the muscles of the legs is only the effect of the pain. Otherwise his condition is good, sleep and appetite nearly normal. There is slight fever. (Morning temperature, 37°·4 C. [99°·4 F.]; pulse, 72; resp., 24. Evening temperature, 38°·4 [101°·2 F.]; pulse, 76; resp., 24.)

In the regions of the face, special senses, and upper limbs, there are neither abnormal sensations nor paralytic phenomena. The lower extremities are, on the contrary, seriously affected in their function. The patient lies in bed with his knees slightly flexed; when asked to straighten his legs he does so slowly, and with expressions of pain. On the left side the movement is more rapid, and the pain less. He brings the extended right limb to a position of slight flexion slowly, and is able with great effort to raise it a little, so that the heel leaves the bed. But when stretched out, he cannot raise it. When the patient now lets the leg fall down again, we notice active contraction of the muscles of the thigh, the sartorius, quadriceps, and vasti. If we try passive motion, the patient does not complain of pain, so long as the movement is not extensive or rapid, but experiences pain on strong flexion; yet we can, without much difficulty, bring the leg to complete flexion. Analogous affections are present on the left side, but less in degree. The left knee presents externally nothing abnormal; the right is slightly swollen—not from any exudation in the capsule, but a slight swelling of the internal condyle of the tibia. On pressure the joint is nowhere tender, except at this point. There is no swelling on the left side, but it is also tender to pressure over the internal condyle of the tibia. In other respects the legs present a perfectly normal appearance, even in respect to their nutrition. The joints of the feet are perfectly moveable.. Sensation in them shows no essential alteration, unless it be an unusual sensibility to slight pricking with a needle on the sole, which readily produces reflex contractions. In the thigh the muscles are tender to pressure, and so are the glutei on the right side. Pressure and tapping on the right trochanter causes pain. The electric irritability of the muscles is perfectly normal.

The abdomen is flat, and quite relaxed. The patient evacuates occasionally, without much pain, a completely purulent stool; at other times, a perfectly normal feculent evacuation occurs; sometimes a mixed one. Examination per anum reveals nothing abnormal; there is no special tenderness here.

The patient was in this state when we saw him for the first time. We found that he was suffering from two diseases—a dysentery which was at that time in the form of white flux, and a painful affection of the lower limbs, with serious functional disturbance, the knees being specially involved. We asked the question whether the second disease was the result of the first, as the fact of its following it in time suggests, and if so, what its nature may be. In spite of the great painfulness of the knee-joint, we could not explain the case on the supposition of a proper inflammation of the joint. There was no exudation, the condyle was only on the right side slightly swollen and sensitive, and passive movements were limited only to a moderate degree. At any rate, we could not ascribe to this affection of the knee the great disturbance of function of the lower limb, the great extension of the pain from the sacrum and the loins to the joints of the foot. We were forced rather to look on the process as a nervous affection—a neuritis sacro-lumbalis,—and we were confirmed in this view by the neuralgic nature of the pain, the tenderness to pressure, the gradual extension of the process from the sacrum to the lower leg, and, lastly, by the hyperæsthesia. We could even explain the affection of the joint as a result of the neuritis. You have in other undoubted cases of neuritis had the opportunity of observing the development of tenderness, and even swelling of the joints.

The relation of this nervous disease to the dysentery we shall inquire into more particularly when we have followed up to the present date the course of the disease.

In respect to the dysentery, the symptoms which then existed were of a somewhat peculiar kind. Without considerable pain, without tenesmus, there were stools of entirely purulent material occurring in the midst of perfectly normal fæcal evacuations. We are able to conclude from this that there was, at some comparatively limited portion of the intestine, a process which led to the secretion of a good deal of pus, while the rest of the bowel had returned to the normal. We were unable by examination per anum to detect such a circumscribed ulceration, but its existence seemed exceedingly probable.

In view of this, injections of nitrate of silver (once a day) and acetate of lead, in doses of 0·02 grain ($\frac{1}{3}$ gr.) every two hours, were ordered, with light diet, and care that the bowels were regularly opened. For the neuritis we ordered an ascending constant current (twenty elements), to be applied for five minutes on each side from the exit of the sciatic to the sacrum.

The treatment was very effectual in the case of the dysentery, and from the 29th November there was no longer any pus in the stools, and the intestine has remained perfectly well ever since. Not so, however, with the affection of the inferior extremities.

On the 27th November we find it noted in the journal that the patient continues to complain of the pain in the knee, which exists spontaneously, as well as on moving. The pain in the upper parts is not confined to the sacrum, but extends down to the coccyx. The right lumbar region, and the two lower intercostal spaces on the right side, are especially painful, and from there the pain extends to the anterior superior spine of the ileum. In this region the skin is painful to the touch, and the tenderness, on pricking with a needle, is distinctly increased. There is also hyperæsthesia in the left lumbar region, but of less degree. We can also recognise pretty considerable hyperæsthesia of the skin over the sacrum and coccyx. The skin of the abdomen shows slight, and that of the right intercostal spaces very lively hyperæsthesia on pricking with a needle. In the leg there is slight hyperæsthesia of the skin, and also of the muscles, on pressure. The movements of the lower extremities are still difficult, slow, and painful. The feet almost trail on the ground, and the patient can only walk when supported by some one. The pain in the knees persists, and is increased by long walking or standing.

The general condition is in essentials good, but there are, almost every evening, slight febrile exacerbations (38·0 to 38·5 C. [= 100·4 to 101·3 F.]); pulse, 72—84.

On December 5th the patient complained for the first time of pain between the shoulders, which was at first taken little notice of, but persisted, and even increased.

On 14th December he complained even more of pain in both knees and between the shoulders, which existed even when lying quiet. In walking—which, though always difficult, is still possible—the pain extends down the lower legs to the maleoli. The vertebræ between the shoulders are also somewhat tender on pressure, and the patient further complains that in moving he feels the left arm painful. The shoulders themselves are only painful on pressing in the neighbourhood of the coracoid process, and here only to a limited degree.

The general health is, on the whole, good. Yet the slight fever persists, and is even increased during the last few days. (M. 37°·6 to 38°, E. 38°·3 to 38°·8.)

January 14th, 1870.—The chief subject of complaint now is the pain between the shoulders. The vertebræ are here pretty tender to pressure. There is no proper stiffness of the vertebræ to be detected, but the patient states that the pain increases when he bends his head forwards; he therefore avoids this movement. He further complains that the pain increases when he moves in bed, especially at night, when he wishes to turn from one side to the other. The pain in the left arm still persists; it extends from the shoulder down to the elbow, occurring also in the muscles of the upper arm occasionally. The elbow (condyles of the humerus) is somewhat tender on pressure, as well as the shoulder and the muscles of the upper arm. Hyperæsthesia of the skin cannot be detected. The pain in the right side of the chest also continues, especially in the region of the lower false ribs. The movements of the arm are in other respects quite free.

In the legs the pain has somewhat diminished, but whenever they are moved they readily pass into a state of tremulousness, with rather lively reflex contractions of the muscles of the thigh.

Slight fever.—General condition unchanged.

It could no longer be doubted that the morbid process which, proceeding from the sacrum, had affected the lower extremities, had now passed upwards, and that the tenderness in the vertebræ and in the inferior intercostal spaces on the right side, as well as the pain in the left arm, was dependent on it; even the fever, which still persisted, must be referred to it, as the intestine had been for some time perfectly well. Without doubt the process had advanced to the spinal cord, and we could conclude with great probability, from the prominent symptoms, especially the hyperæsthesia and fever, that we had to do with spinal meningitis. In accordance with this view we prescribed repeated blood-lettings over the vertebræ, and the inunction of Ung. ciner., with the continuous use of small doses of iodide of potassium.

The eleventh inunction has been made to-day (January 16th), and although not yet well, there is an essential improvement in his condition. He is a good deal out of bed, walks about alone without difficulty, although still somewhat clumsily, especially at first; he has been for several days quite free from fever. The pains in the knees, as well as between the shoulders, still persist, but they are less severe, and the pains in the back, in the side and arm, have quite disappeared. The tremulousness of the legs also occurs but seldom, although their movements do not yet possess their normal activity. The right knee shows still the slight swelling of the internal condyle, but is less tender on pressure as well as in walking.

You will be convinced, by the course of the disease, of the correctness of our earlier view, that the process is a nervous one; we have not to do with a primary affection of the knee-joint, but, on the contrary, this depends secondarily on the nervous affection. This latter took its origin in the sacrum, extended to the lumbar region, thence to the inferior extremities, affecting especially the right limb, the knee-joint participating, but the disease extending down to the maleoli and articulations of the foot. It was distinguished by spontaneous attacks of violent pain, with periodic exacerbations, moderate hyperæsthesia, increased reflex irritability of the muscles of the thigh, and considerable functional disturbance in the lower limbs. From these symptoms we have sufficient foundation for designating the disease at its beginning as a neuritis migrans (lumbo-sacralis and ischiadica). It has doubtless passed afterwards to the spinal cord or its meninges, and has affected them, not very intensely, but distinctly enough, and has finally extended to the neighbourhood of the upper dorsal vertebræ.

The disease is at present retrograding. We may hope that the improvement will be permanent, and that the patient will go on to recovery if no intercurrent exacerbations interfere. After the complete removal of the inflammatory phenomena (and we propose to push the inunction to slight salivation) we shall, continuing the careful use of the iodide, return to the continuous current so much approved in neuritis, and we hope to remove what remains of the disease in a few weeks.

Now that we have considered the symptomatology and diagnosis of this case by itself, we pass to the discussion of the question, whether the second affection, the neuritis migrans and meningitis, has any connection with the first, the dysentery, and, if so, how? The occurrence of the former at a time subsequent to the dysentery renders it *à priori* probable that such a connection exists; but how far can we base such a conclusion on known facts? Do paralytic conditions occur after dysentery, and of what nature are they?

We have, so far as I know, only occasionally heard of such affections of late years. But according to older observations, it is an undoubted fact that paralysis occurs not at all infrequently after dysentery. Joseph Frank quotes some observations of this kind, and refers especially to the dissertation of Fabricius, Helmstedt, 1750: "Paralysis seu hemiplegia transversa resolutionem brachii unius et pedis alterius lateris exhibet. Rara hæc paralyseos forma a Conrado Fabricio descripta post dysenterias malignas epidemicas, adstringentibus et opiatis præmature suppressas observata est." Observations were afterwards recorded by Graves, in which, after colics and inflammation of the intestine, paralysis occurred; and English veterinary surgeons have observed cases among horses and cattle where paralytic weakness of the posterior extremities had followed inflammation of the intestine. Similar observations were recorded by Leroy d'Etiolles, in his prize essay (Paris, 1856), and others by Gubler.*

According to these, there can be no doubt that sometimes in consequence of dysentery, as well as of other forms of inflammation of the bowel and colics, paralysis occurs, which usually is confined to the inferior extremities, but may extend upwards, involving the superior extremities, in these latter cases taking by preference the form of paralysis transversa. There is not much said further as to the symptoms under which this paralysis occurs, but it is stated that sometimes it appears suddenly without pain, but that more frequently it is preceded by acute pain in the legs. As to the period of its occurrence Joseph Frank states that it commonly begins after the dysentery has passed off, as well as when the latter has been too early suppressed by the use of astringents and opiates. As we now no longer believe in the danger of the suppression of morbid secretions, we will attach little weight to the last statement of Joseph Frank. But we find the first part of his statement confirmed in our case, for the dysentery was all but cured when paresis of the legs appeared. In other respects also we find that the time of occurrence in our case corresponds with the earlier observations; and we may even mention, as confirmatory, the remarkable circumstance that our patient showed an indication of paralysis transversa. For while the right leg was much more involved than the left, the right arm remained perfectly free when the left showed distinct evidence of being involved in the morbid process in the spinal cord, though not very severely.

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* "On the Paralysis occurring after Acute Diseases." Archiv Génér., 1860, i. and ii.

We are therefore justified, on the ground of earlier observations, in considering the paralysis as a result of the preceding dysentery. But we have already assumed a definite anatomical process as at the basis of this paralytic affection. We have concluded from the symptoms that an ulceration of the rectum had occurred as a consequence of the dysentery, and that pus was discharged from the ulcerated surface, sometimes without any fæcal admixture in the earlier periods, and without the tenesmus peculiar to dysentery. We may suppose that the inflammatory process had passed from this ulceration to the surrounding cellular tissue of the pelvis, and had finally led to the neuritis lumbo-sacralis. This is indicated by the severe sacral pain accompanied with fever, and extending to the loins and hip, and finally to the legs. The grounds on which our diagnosis of neuritis and meningitis is founded we have already sufficiently stated.

In this diagnostic interpretation of the symptoms we are not quite in correspondence with the views of earlier authors. But similar conclusions have been expressed by several writers, and it seems to me that we can find in a thorough analysis of the symptoms in this case a not unimportant confirmation.

The original views of the nature of dysenteric paralysis by no means correspond to ours. No changes could be recognised in the spinal cord when opportunities presented themselves of examining it. Leroy d'Etiolles therefore enumerates it among the Paraplegiæ, which occur without disease of the cord. Gubler describes it among the paralyses occurring after acute diseases, analogous to those after diphtheria, typhus, cholera, etc. It was enumerated by English authors among the reflex paralyses, also on the ground of the negative results of inspection of the cord.

We come here upon an important and interesting subject in nervous pathology, but one which extends beyond dysenteric paralysis and includes some other forms.

When Marshall Hall established the theory of the excito-motor nerves, or the reflex function of the spinal cord, he did not hesitate to make use of his new doctrine for nervous diseases. It was easy to refer some forms of convulsion to this reflex function. But, in his work on the "Diseases of the Nervous System," he drew attention to some cases of paralysis whose origin he accounted for in a similar way. In this way he interpreted the case of a child of two years, who, in consequence of teething, was affected with paralytic phenomena in the arm. In a similar manner Dr. Veuldy referred infantile paralysis to the irritation of the teeth or the intestines. While, however, these views remained unnoticed, certain observations by Stanley soon afterwards attracted a great deal of attention, and led to the general acceptance of this theory. E. Stanley contributed to the "Medico-Chirurgical Transactions"* a series of observations which were intended to show that symptoms of disease of the spinal cord, and especially paraplegia, may develope in consequence of affections of the kidneys and bladder, and that after death the cord will be found perfectly normal, with the exception of a moderate hyperæmia. The author says, in a note to Case V.: "In this case we observe the same phenomena as in the four cases previously related, namely, inflammation of the kidneys, with paralysis of the lower limbs, but occurring under such circumstances as were not likely to lead to an opinion that the symptoms originated in disease of the spine." The first four observations of Stanley leave in considerable doubt the correctness of this conclusion. They are cases of paraplegia, with suppuration of the bladder and kidneys. On post-mortem examination there was the usual form of suppurative nephritis, and the spinal cord appeared normal. But this latter circumstance, especially without a full microscopic examination, is not sufficient proof that the cord was actually normal, and it is exceedingly probable that we have here to do with myelitis, with the usual results of suppuration of bladder and kidneys. The fifth and sixth cases, however, deserve the importance which has been attached to them. The fifth case is that of a man who, in consequence of a gonorrhœa which had been stopped by injection, was affected with retention of urine; the bladder and sphincter became paralysed, and the lower extremities lost, to some extent, their power of motion. Severe pain developed in the back, corresponding to the fifth lumbar vertebra, and after some time the inferior extremities were completely paralysed, with almost complete anæsthesia. The patient died, and there was found enlargement and softening of the kidneys, with numerous small deposits of pus. The brain and cord appeared normal. The sixth case is that of a man, 30 years of age, who suffered from gonorrhœa and phymosis. He was suddenly attacked with paraplegia as high as the umbilicus, with complete loss of motion and nearly complete of sensation. Pain in the loins had preceded this for a day or two. After death there was found congestion of the vessels in the lumbar region of the cord, but otherwise nothing abnormal. In the seventh case we have only a man, 35 years of age, who suffered from incontinence, and had for two years severe pain in the back, at the level of the sixth dorsal vertebra. The pain extended over the chest, and was accompanied with difficulty of breathing. No paralysis. After death the pelvis of the kidney was found dilated and filled with pus; the cord was healthy.

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* Vol. xviii. p. 260, 1833, "On the Irritation of the Spinal Cord and its Nerves, in connection with Disease in the Kidneys."

These cases, especially the fifth and sixth, show that we may have developed, in connection with diseases of the bladder and kidneys, forms of paralysis which have all the characters of spinal paralysis, and which may lead to death. These forms have been called Paraplegia urinaria, or uro-genitalis, in reference to the disease to which they are related. As to the cause of their occurrence, the fact that on most careful examination the cord seemed to be normal, appeared to be much in favour of considering them reflex. Hence the name and the theory of reflex paralysis. But as this theory was as yet unsupported by any physiological observations, it was a welcome circumstance when Comhaire, in 1840, in the course of some experiments instituted for a quite different object, made an observation which seemed to fit in exactly with these pathological observations. In his dissertation* he states, that after extirpation of the kidney in dogs, he has always found a distinct weakness of the legs on the side operated on.

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* "Sur l'exstirpation des reins." Paris, 1840.

Stanley, in his paper already referred to, cites the observations of Hunt, who had also observed four cases of kidney disease in combination with spinal symptoms. He also calls to mind cases of uterine disease, in which there may be such paralysis of the lower limbs as to confine the patient entirely to bed.

The theory of reflex paralysis was especially supported by Graves,* whose observations on the paralysis following colics and inflammations of the intestine we have already referred to. Graves calls attention to the fact that pain, convulsion, and loss of motion may be produced by causes which act on the peripheral ends of the nerves; that such actions might be propagated to the central organs, and thence produce by reflex action similar phenomena in other parts. Take, for example, the amaurosis from the action of cold on the facial branches of the fifth, and as belonging to this class also the paraplegias which occur in consequence of affections of internal organs, especially those of the abdomen.

In Germany this theory was represented by Henoch;† but it was especially formulated by Romberg, and it obtained, through his well-known "Handbook of Nervous Diseases," an effectual introduction to the medical public. In the edition of 1846 the author brings forward, as a physiological confirmation of reflex paralysis, the fixity of the features after section of the fifth, and the immobility of the pupils after division of the optic nerve. The altered condition of the movements of the eyes in amaurosis, and the dumbness which follows anæsthesia of the acoustic are to be remembered as pathological instances. "It is most difficult," he proceeds to say, "to determine the sources of reflex irritation in the domain of the sympathetic, whose provocative influence on spinal nerves has been proved both by experiment and observation." "The stimulating and invigorating influence on our power of movement exercised by the hygienic activity of our internal organs, has not yet been sufficiently recognised." We are reminded of the activity or sluggishness of the limbs in a regular or irregular state of the bowels, and the origin of reflex paralysis is deduced from the withdrawal of this stimulating influence on the part of the sympathetic. Romberg, with the English writers, recognises three forms of reflex paralysis—1, those arising from affections of the intestine; 2, those depending on diseases of the urinary organs; and 3, those referable to affections of the generative organs.

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* "A system of clinical medicine." Dublin, 1843.

† "Vergleichende Pathologie der Bewegungsnervenkrankheiten der Menschen und der Hausthiere." Prize essay, Berlin, 1845.

In this way the theory of reflex paralysis was more distinctly formulised than before, it was also made somewhat artificial. It was, in fact, only admissible on the assumption that these three classes of secondary paralysis of the cord were not due to an anatomical process, but required to be referred to some reflex processes. As a matter of fact, however, this presupposition was only supported by the circumstance, that on post-mortem examination of such cases, as in those of Stanley and Graves, the cord appeared to be perfectly normal. Hasse and Valentiner have with perfect right raised the objection to the statements of Romberg, that the observations on which they are based are exceedingly imperfect, inasmuch as they were in no case made with the aid of the microscope; and it is notorious that not infrequently spinal cords which are much diseased appear to the naked eye perfectly normal. Moreover, it is not possible to deduce any safe conclusion from the experiments of Comhaire, because the weakness of the legs mentioned by him is a result of the injury, and may be produced by a simple division of the lumbar muscles, without extirpation of the kidneys.

Both of the series of facts adduced in confirmation of the reflex theory were thus seriously shaken; and Romberg did not hesitate, in the third edition of his Handbook, to give up his former views as wanting in foundation. The theory of reflex paralysis lost in him a powerful authority, but it was not altogether given up. Although there were no positive experimental facts as yet in its favour, still the advance of our knowledge of the functions of the nervous system gave new and varied support to some such connection as that which Stanley, Graves, and Romberg had supposed. Particularly the discovery of the inhibitory nerves had this effect, demonstrated in the case of the vagus by Weber, in that of the splanchnic by Pflüger, and in the superior laryngeal by J. Rosenthal. We became acquainted with nerves whose action consisted in the inhibition of the action of other nerves. Reflex paralysis and reflex inhibition appeared very similar processes, and so the possibility of reflex paralysis was again rendered more probable. This has had all the more weight, as inhibitory processes came to be recognised in the cord itself. We must leave out the consideration of these processes here, especially as no perfect agreement has yet been attained as to them. The facts which have been proved, however, by Setschenow, Herzen, Golz, Nothnagel, Lewisson, etc., are these, that by strong irritation of peripheral nerves, the regular function of the cord (conduction and reflex action) may be completely interrupted for a time. The more recent work by Lewisson which treats of reflex paralysis, falls back on these observations.

Before we enter on the consideration of these, however, we must, in order to keep to chronological sequence, refer to another theory of reflex paralysis brought out by Brown-Séquard in 1856. Under this designation he includes a large number of cases of paralysis, especially of the lower limbs, which arise from diseases of the uterus, ovaries, kidneys, bladder, and intestine, as well as the lungs, toes, knees, and peripheral nerves. On post-mortem examination, the cord appeared in no case diseased; but only in two cases was any microscopic examination made. Brown-Séquard believes that these paralyses were due reflexly to the peripheral irritation, *i.e.* through a reflex contraction of the blood-vessels. If the reflex contraction of the vessels affects the spinal cord, we have paralysis; if it affects the muscles, we have atrophy of these. The author bases this theory on experiment. When he ligatured the pelvis of the kidney in such a way as to irritate its vessels and nerves, then he observed in the pia mater of the cord a contraction of the vessels. This theory has received little support, and it will be acknowledged that a reflex contraction of the vessels can hardly lead to a serious paralysis, which may even have a fatal issue. Jaccoud, who opposes Brown-Séquard's theory, supposes that anæmia does not produce paralysis, but convulsions; but this objection is met, at least in the case of persistent anæmia, by the observations of Stenson. Jaccoud's own theory can also receive very little support; his view that the irritation of the inflamed bladder or kidney exhausts the centres in the cord—that is to say, tires them out, and thus produces persistent paralysis, is almost without analogy in the records of physiology and pathology.

On the other hand, the experimental facts mentioned above, as to the reflex inhibitory centres in the cord, have induced observers to look for an analogy in reflex paralysis. Dr. Lewisson,* of Berlin, has done this, but on the understanding that the analogy only applies to functional disturbances of the spinal cord. His experiments also showed that violent irritation of the centripetal fibres may interfere with the normal functions of the cord, producing a paralytic weakness. Lewisson was not able to confirm the experiments of Comhaire, whose conclusiveness we have already mentioned as doubtful. In ten experiments on rabbits and two on dogs, he observed no paralytic symptoms after removal of the kidneys. But when Lewisson drew out the kidney through the wound, and pressed it firmly between the fingers, he observed a complete paralysis, with loss of the reflex irritability, which lasted as long as the pressure continued, and generally a short time longer. At the same time the irritability of the sciatic nerve to the electric current remained intact. In the same way he was able to produce paralysis of the lower limbs, by crushing the uterus in rabbits, and also, on several occasions, by crushing a loop of intestine four centimeters long, and the urinary bladder, which had been previously emptied. The paraplegia induced in these ways always disappeared at once, and the centres were found intact after death, which usually occurred from peritonitis. Lewisson, from these observations, claims that the existence of reflex paralysis has been justified. There is no doubt that his experiments prove more distinctly than the earlier ones, that paralysis of the cord may be produced by peripheral irritation. But it is not to be forgotten that this paralysis only occurred as the result of violent irritation of the internal organs, that it occurred very quickly, and disappeared very soon after the cessation of the peripheral irritation. If we desire to compare these experiments with the reflex paralysis of Graves and Romberg, then we must first prove that the phenomena correspond. In the first place, the assumption, which even

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* "On the Inhibition of the Activity of the Motor Nerve Centres, by Irritation of Sensory Nerves." Reich. and Du B.'s Archiv, 1869, pp. 255-266.

Lewisson makes, that the paralysis is functional, is by no means universally acknowledged. The cases in which the anatomical alterations have been proved to exist after death, are now as numerous as those with negative results, and the latter must always admit of doubt. But apart from this, it is exceedingly doubtful whether we can compare the primary diseases of the intestine, bladder, or uterus with the violent crushing used by Lewisson; and it is still more doubtful if we can compare it to a continued crushing, without supposing that the irritability would diminish, and the paralysis finally disappear. On the contrary, the paralyses persist in the cases recorded, even after the primary disease is cured. A sudden disappearance of the paralysis has never been observed. Again, the onset of the paralysis is very seldom sudden; it is generally preceded by pains, weakness, and dulness of sensation. The correspondence with Lewisson's experiments is, therefore, not very great, but yet it is not inconceivable that the two conditions may be referable to similar processes. At any rate, these experiments render the possibility of the occurrence of reflex paralysis exceedingly probable, and give it an experimental foundation. There occur cases of paralysis of the lower limbs, sudden in their onset, and disappearing after a short time, which happen, it may be, as a consequence of violent diarrhœa or colics. Such cases may very probably be explained as actual instances of reflex paralysis; but we have few complete observations in which the commencement, symptoms, and course have been carefully noted.

The majority of cases of secondary paralysis show so much divergence from the experiments of Lewisson, in their origin, symptoms, and course, that it seems useless to attempt to explain them in that way, and we must approach the question from a different side. We have already seen that there have always been some opponents of the reflex theory. They sought for a material connection between the primary and secondary diseases, in the anatomical extension of the process. This view is, in fact, much older than the other, and was only put aside on the discovery of reflex action. Troja says, in 1780, "A violent inflammation of the kidney may induce an irritation of the nerves of the kidney, which may extend through these to the spinal cord; in this way paralysis of the lower limbs, with loss of sensation and motion, sometimes arise, and may lead on to death."

And more recently, when so much doubt had been raised as to the theory of reflex paralysis, a return was made to the older view, and it was sought to establish an anatomical connection between the seat of the original disease in the abdominal cavity and the cord.

Among the more recent opponents of the reflex theory is Sir William Gull,* who also opposed the vasometer theory of Brown-Séquard. He asserts that the "urinary paraplegia" occurs almost exclusively in men who have suffered for years from diseases of the bladder or stricture of the urethra, and in whom it is to be presumed that the nerves of the mucous membrane are already blunted; it almost never occurs in women and children. The negative result of post-mortem examination is of no importance, as microscopic investigation was seldom made. Gull is of opinion that the inflammation of the urinary passages is propagated, *per continuo*, to the cord, and that this is particularly easy in man, from the complicated arrangements of the pelvic organs. In support of his view, Gull describes a case in which paraplegia occurred in consequence of gonorrhœa and syphilis, and on post-mortem examination the cord appeared to be normal; on more careful microscopic examination, however, pretty extensive fatty degeneration was found beneath the level of the sixth dorsal nerve.

Remak,† also, refers the paraplegia urogenitalis to neuritis, but in a somewhat different way. He states that he has seen a large number of cases in which the paraplegia was caused by disease of the larger nerve-trunks, mostly of an inflammatory nature, and he considers that most of the cases given by Leroy are of this kind. The cases of paraplegia, often of high degree, connected with paralysis of the bladder and subsequent nephritis, are caused by a lumbo-sacral neuritis, and curable by the constant current, although they have been formerly referred to disease of the brain and cord. This neuritis descendens beginning in the lumbar and sacral nerves, extends even to the plantar branches, and is accompanied by violent pain, and frequently œdema. The tenderness on pressure distinguishes this neuritis from myelitis. On the other hand, myelitis with neuritis occurs as the cause of paraplegia with paralysis of bladder and rectum.

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* "Med.-Chir. Trans.," vol. xxxix. 1856.

† "Med. Central. Ztg.," 1860, No. 21; and "Oesterr. Zeitschr. f. praktische Heilkunde," 1860, Nos. 45 and 48.

In the year 1863, Kussmaul* recorded a case of paraplegia urinaria in which he detected, on post-mortem examination, besides an atheromatous condition of the pelvic arteries, a partial fatty degeneration of the nerve-tubes of both sciatic nerves. He considers that the cause of this latter lesion is the chronic inflammation of the pelvic cellular tissue, and the propagation of this to the sheath and tubes of the nerves.

I have also in my introductory address† recorded three cases of paraplegia urinaria, of which two died and were proved to be cases of myelitis. The third, which was a case of vesical calculus, corresponded so closely with the others in its symptoms, that it also may be set down as myelitis. This myelitis begins with symptoms which indicate that the lumbar enlargement of the cord is its original seat. It may be confined to this situation and remain incomplete, as in the third case; or in the severer cases it may lead on to perfect paralysis, and extend further upwards. The symptoms in the three cases were essentially similar, and may be summed up as follows:—After an affection of the bladder had existed for some time, there occurred prodromal symptoms in the form of pain, sleepiness, weakness of one or both legs, and then, with comparative suddenness, paraplegia supervened. This involved both motion and sensation, and was at first accompanied by increased reflex irritability, while the electro-muscular irritability remained intact; there were also occasional attacks of violent pain in the legs. The paralysis was confined to the lower limbs and the sphincters, and the disturbance of sensation at first extended to the inguinal region, becoming less from below upwards. These symptoms were referable to a rather limited affection in the upper part of the lumbar enlargement. The disease of the cord, therefore, began with a circumscribed affection at the point where the vesical nerves have their entrance and exit. It is natural to suppose that the disease had extended along the nerves from the diseased bladder to the cord. Yet this supposition was not confirmed post-mortem. In both of the fatal cases there was an intense myelitis, extending pretty widely—in one of the cases even to the brain—and characterised partly by softening and suppuration, partly by the presence of fatty granular cells. It was most intense in the lumbar region, in which it had begun, but no distinct relation to the vesical nerves could be discovered. So that the propagation of the inflammatory process—however probable it may appear from the course of the symptoms—has not been proved. We may, indeed, question whether the extension of a neuritis to the cord, and the production in that way of a myelitis, has ever been demonstrated.

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* "Würzb. med. Zeitschr.," iv. 56—63.

† Königsberg, 1865.

This gap in our knowledge has been, in great part at least, filled up by an interesting experiment which one of my students has recorded in his Inaugural Dissertation.* In his experiments on neuritis he irritated in various ways the sciatic of the dog and rabbit. In one of these experiments, the rabbit became paraplegic, and died in three days. On examination there was found a collection of pus at the place where the nerve had been irritated, and a similar lesion within the spinal canal exactly at the spot where the root of this sciatic enters the spinal cord. The cord was much softened at this part, and contained pus-corpuscles and fat-granules. The nerve-trunk between the two collections of pus showed nothing abnormal. This observation is an experimental proof of the fact that a local neuritis may induce an acute myelitis with complete paraplegia. It therefore renders probable the progressive inflammatory process mentioned above, even in cases where a neuritis is not directly made out.

It results, then, from these considerations, that in a considerable number of cases of paraplegia urinaria, an anatomical affection of the cord has been proved, in the form of a myelitis, which begins in a circumscribed lesion of the upper part of the lumbar enlargement. From this, and from the symptoms of lumbo-sacral neuritis which not infrequently precede, it appears probable that the inflammation of the bladder first passes to the nerves, and induces there a progressive neuritis. The experiment cited shows that this may lead to myelitis. This may therefore be looked upon as the most natural and frequent mode of occurrence of paraplegia urinaria. It is not yet determined whether, in addition, an actual reflex paralysis does occur; and this is in no way contradicted by physiological and experimental facts. It may be supposed to exist in those cases in which paralysis occurs in consequence of a violent painful irritation of the bladder or the intestines, perhaps also of the uterus, where the affection occurs rather suddenly, is not accompanied by excruciating pain, and where the primary affection only lasts a short time, and so does not lead to the secondary process becoming independent.

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* Tiesler, "On Neuritis." Königsberg, 1869, p. 25.

The same processes are applicable to the other forms of paraplegia, which used to be reckoned as cases of reflex paralysis. Returning to our own case, we see in it an exact counterpart of the ascending neuritis and affection of the cord which follow the diseases of the bladder. In our case we were able to conclude, from a careful observation of the symptoms, that the dysenteric process had advanced to the connective tissue of the pelvis, and had produced a neuritis sacro-lumbalis, which afterwards involved the cord, and extended up to the cervical region. The symptoms and the analogy with the paraplegiæ urinariæ, afford sufficient evidence of the nature of the process.

If, finally, we inquire whether an analogous process exists in the case of the third class of supposed reflex paralyses—those associated with affections of the uterus—we shall find that here the matter is complicated by the occurrence of hysterical paralysis, and we cannot suppose that there is any anatomical alteration in that affection. But we may ask whether a neuritis migrans may develope in connection with the female organs of generation, which leads to paralytic, and particularly to paraplegic, symptoms? This question is also to be answered in the affirmative. I shall now show you a patient in whom, after a prolonged retroflexion of the uterus, symptoms of ascending neuritis ilio-lumbalis sinistra developed, followed by a neuritis descendens cruralis, with distinct paresis and emaciation of the corresponding limb. There supervened on this a slighter affection of the right leg, and the most manifest hysterical phenomena. But the original neuritis was distinguished from these later occurrences by the persistence and definiteness of the symptoms, and the resulting emaciation of the muscles. According to the experience of my respected colleague, Professor Hildebrandt, this process is not infrequent in connection with flexions of the uterus. We have, therefore, perfectly similar processes occurring in uterine paralysis to those which we met with in dysenteric and genito-urinary cases.

I shall read to you the record of the case, and then we can examine the patient.

L. Fischer, a working woman, 27 years of age, was healthy as a child, with the exception of an attack of hooping-cough. She first menstruated in her 13th year. The discharge was at first profuse, but ceased in her 15th year with the development of chlorotic symptoms. She suffered much from headaches, pain in the back, palpitation, and repeated swelling of the legs. Menstruation returned in her 16th year, and these symptoms diminished in intensity. Menstruation, however, was seldom regular, was often delayed, and at other times would occur every fortnight. In her 17th year, patient had an abdominal inflammation, which was treated with leeches and poultices, and lasted six weeks in all. Next year she passed through an attack of intermittent fever, which readily yielded to the use of quinine. In the same year she is said to have had two attacks of laryngitis and diphtheritis. In the winter of 1866–67 she had a severe attack of hoarseness, which increased to complete loss of voice, and only disappeared after prolonged treatment. Almost simultaneously occurred, for the first time, violent pains in the hands and feet, which were treated by blood-letting (venesection and leeches). In July, 1867, as patient was attempting to carry a sewing machine downstairs, she fell violently on the buttocks; immediately afterwards she experienced severe pain in the abdomen, and retention of urine, which symptoms continued, with some little change, till February of the following year. About this time patient had an attack characterised by fever, vomiting, and delirium, for which she was treated with cold applications to the head, warm poultices, and the inunction of mercurial ointment into the abdomen. She remained ill over six weeks. After this illness, there remained persistent pains in the abdomen, which were increased by moving, pressure of the clothes, etc. On this there gradually supervened, without the patient being able to assign any exact date, a certain weakness of the left leg, which was specially prominent after violent exertion, walking, etc. At the same time there occurred, occasionally, pains in both knee-joints, and in both legs, but more strongly in the left; these pains radiated from a tender spot in the sacrum, and extended upwards to the region of the lower ribs on the left side. In November, 1867, patient, on account of the state of her health, passed into a state of mental excitement, and had a convulsive attack, which, according to her own account, lasted three days, during which she only recovered her senses at short intervals. When the convulsions had disappeared, and patient attempted to leave her bed, she noticed a paralysis of the left leg, which entirely prevented her from walking. The convulsions have occurred frequently since then, sometimes several times a day, especially after mental excitement. The patient remarked that almost every convulsion is preceded by a violent dragging of the back of the head towards the neck, and a feeling of cold in the back and extremities. In January, 1868, a fresh convulsive attack occurred, which lasted a whole day, and was immediately followed by paralysis of the right leg. At this time the patient could only walk through the room with the assistance of two crutches, and even this produced violent pains in the legs, especially the feet and knees. The functional disturbance was always greater on the left than the right side. The patient was treated from February till the end of July in the Gynæcological Clinique. On examination the following was noted:—Hymen present; uterus situated higher than usual; vaginal part swollen; body of uterus retroflexed; the sound shows that the uterus is increased to three inches. The neighbourhood of left ovary is painful, and the pain sometimes increases to such a degree as to necessitate the application of leeches. General convulsions occur from time to time.—The paresis improved somewhat, but the left leg especially remained weak, and the patient complained almost continuously of dragging pains in it.

On the 3rd December the patient was sent to the medical side for galvanic treatment.

Present state.—You see that the patient is a strongly-built, muscular woman, with lively red cheeks and lips. She is perfectly free from fever, but lies for the most part in bed, and complains of gnawing and pricking pains in the abdomen, particularly the left hypogastric region. These pains are pretty constant, but with frequent exacerbations. She complains, besides, of pains in the back, which are worse during menstruation, and of pains in both knees and feet, especially when the feet are cold. Now and again moderate pains in the hips occur In the joints of the upper extremities also, when they get cold, pains are experienced. These pains appear to be associated with a subjective feeling of cold, even when the patient lies in bed. Her hands and feet are generally cold.

The chief complaint of the patient is of the weakness of the lower limbs, especially of the left leg, which prevents her walking through the room without crutches. On external examination it appears that the left leg is thinner than the right; and this is true of the thigh as well as the leg. (Circumference of left calf, 35·3; right calf, 37·5; middle of left thigh, 44; of right, 47 centimetres.) Both legs feel rather cool, but equally so. The left side of the pelvis is raised, so that the left leg seems shorter. Pressure on the left trochanter, as well as on the femur, causes considerable pain, and so does pressure over the fibula and calf. Passive movements of the joints cause no pain, but she is very sensitive to pressure over the sciatic notch. The right leg is in most respects in a condition similar to the left, but the pain is less. The active movements of the legs are impaired; they can only be raised slowly and imperfectly, especially the left. The joints of the knee and foot, including the toes, are freely moveable. Sensation shows no considerable alteration, but patient states that she feels the pricking of a needle rather less on the left leg than the right.

Patient is not able to walk without support, as her knees give way under her. She can only walk with a crutch and stick. She limps much in her gait, the shortened left foot being hardly set down.

You can understand from this history how a very strong girl, who has only had a temporary attack of chlorosis, may develope a uterine affection with retroflexion and parametritis or oophoritis of the left side, and how, in consequence of this, the most pronounced nervous affections may occur. From the first, a painful affection of the left leg was remarked, which was directly connected with pains in the abdomen, in the sacrum, and the left lower ribs, and which led to functional disturbance and emaciation. It is distinguished from hysterical paresis by the absence of any considerable alteration of sensation, and by the constancy of the symptoms. Looking to these symptoms, we must set it down as neuritis sacro-lumbalis and cruralis. A similar process has developed in the right leg. In connection with this nervous affection, we see that a series of hysterical symptoms have developed—headache, vomiting, aphonia, and general convulsions, which are essentially different from the symptoms of the neuritis in their changeable character.

After the treatment of the uterine affection has been set agoing, the neuritis requires independent galvanic treatment, which we shall begin here, and we hope that we shall succeed in improving the paresis, if we do not completely cure it.

We have still to mention that M. Benedict, of Vienna, has described another form of traumatic reflex paralysis, which does not result from affections of the abdominal organs, but directly from diseases of the nerves. This form developes, according to him, in widely separate regions of the extremities. The most distinct case among those recorded is the following :—A builder, 55 years of age, sustained a fracture of the neck of the right femur. The fracture healed with a false joint, and was the source of continual pain. During treatment, paralysis occurred in the upper limb of the same side, with atrophy of the triceps, paralysis of the biceps, considerable paresis of the extensors of the carpus and fingers, of the interossei, and, lastly, of the extensor and adductor pollicis. Benedict found that he could produce reflex movements in the paralysed parts by acting on certain points in the neighbourhood of the fracture, and he explained the affection as a traumatic reflex paralysis. By local electric treatment, and reflex irritation, the affection of the upper extremity was cured. These symptoms render it extremely improbable that the paralysis of the upper limb in this interesting case was, in the proper sense, reflex. Even the atrophy of the muscles serves to indicate some process in the cord or nerve-trunks. It is possible at least to guess at the kind of anatomical process which would be produced by the continuous irritation of the nerves at the situation of the false joint, and which might lead to the secondary affection of the arm. We cannot fail to recognise an analogy with the symptoms of neuritis migrans; but we cannot with certainty assert that the neuritis may be propagated through the cord.

I can only state, in reference to this point, that I have seen several cases in which the symptoms of neuritis passed from one lower limb to the other, or from a lower limb, in the course of time, to an upper, or *vice versâ*. We have, however, no certain knowledge of the anatomical process which lies at the basis of such propagation.

We have now, Gentlemen, passed in review the various forms of disease which have been described as reflex paralysis. The fact has been incontestably deduced, from a thorough analysis of these observations, that in consequence of various inflammatory organic affections, a paralysis may develope which, from its symptoms, is referable to an affection of the cord. This simple fact is of great interest in relation to the origin and mode of extension of nervous diseases. Important pathological theories and discussions have grouped themselves around this fact, and these have led us to attain to a fuller insight into the relations of these processes. We have attained to the result, that the existence of a true reflex paralysis is rendered probable, by physiological experiment, but has not yet been sufficiently proved by pathological observation, and that the most frequent mode of origin of these affections is doubtless the progressive inflammation of the nerves, which not infrequently passes into the spinal cord. We recognise in this tendency to the propagation of the process, a peculiar characteristic common to the nerves and the cord, and one which is of great interest and importance in the generalisation of nervous diseases.

The views we have attained to, as to the nature of the secondary paralytic affections, are, of course, of great influence on *treatment*. They induce us to use an independent treatment for an independent affection; and this would be quite superfluous if the disease were reflex. The *indicatio causalis* must always be attended to first. We must always, where possible, cure the original disease, to begin with, whether it consist of an inflammation of the bladder, or dysentery, or disease of the uterus, or neuritis. By the cure of these, the source of the secondary disease is often cut off, and the latter may immediately be reduced in intensity, or may remain stationary. On the other hand, it very readily increases in intensity and obstinacy with the continuance of the original inflammation.

At the same time, we do not always observe such a close connection. We not infrequently observe that the secondary paralysis persists, and even gains in intensity, though the primary affection is cured. And, *per contra,* if the primary disease is incurable, we need not give up hope of meeting the secondary affection.

The second indication, therefore, is to treat the secondary disease itself. And here the first question is—What is the nature of the process? From what has been said already, you will conclude that the diagnosis of the processes concerned is not so difficult, and that must form the basis of our treatment. If we have an actual reflex paralysis, then we should expect to cure it at once by diminishing or removing the original irritation, and no other interference should be necessary.

If, however, we have to do with an anatomical propagation along the nerves, then the treatment is of much greater consequence. If the cord has become affected, the prognosis is serious, but by no means very bad. There have been a considerable number of cases of recovery and improvement recorded, even by the older authors, as well as fatal cases. According to the intensity of the inflammation of the cord, we prescribe blood-letting, rest, inunction of mercurial ointment, and we further recommend the use of iodide of potassium and electricity.

All recent authors who have studied neuritis attest the good effects to be obtained from the use of the constant current in this disease; among these, Remak is to be specially mentioned I am able in great part to confirm these statements, from the results observed in our clinique, and I consider this our most effectual therapeutic agent. But even when a neuritis has gone on to involve the cord, the constant current is still of service. In this case it is necessary to remove or diminish the inflammatory irritation by blood-letting and mercurials, to begin with; but we can then attain to very encouraging results by the careful and consistent use of the constant current (15–20 elements, without reversing the current, for six to eight weeks, or longer). I hope to attain some result from the use of galvanism in the two cases which I have brought before you. I expect to cure the male patient completely, and I trust that the suffering of the hysterical female will be still further mitigated.

ON BRONCHIAL ASTHMA.

BY

A. BIERMER,

ZURICH.

GENTLEMEN,—We have just had the opportunity of examining together a characteristic case of bronchial asthma. You will have convinced yourselves by this observation that we can most easily explain the objective pulmonary symptoms during the asthmatic attack, if we suppose the existence of a spasm of the bronchi.

I have on previous occasions pointed out, *that the interpretation of the so-called bronchial asthmas is to be found in the expiratory disturbance, and that the much prolonged and forced expiration with the sibilant râles implies an obstruction in the middle and smaller bronchi, which almost necessarily forces us to the supposition of a spastic contraction of the bronchi.* I think I have given you further evidence of this to-day, especially as it was possible in this case to adduce, by means of a new remedy, a therapeutic confirmation of the spasmodic nature of the respiratory obstruction.

You will remember that we saw our patient a few minutes ago at the acme of an asthmatic attack, with the most distinct signs of obstructed expiration, we saw evidence of forced and prolonged expiration, with sibilant râles, over-distension of the lungs, depression of the diaphragm, and so on. But the scene changed under our very eyes after the administration of chloral hydrate.

The asthmatic respiration, the râle indicative of stenosis of the bronchial branches, the depression of the diaphragm, in short, all the signs of obstructed respiration and over-distension of the lungs connected therewith, disappeared in from five to seven minutes after the administration of the chloral. The respiration had become quiet and free, and in place of the auscultatory signs of stenosis, we found almost everywhere the normal respiratory sounds.

Are we not warranted in taking the rapidity with which the scene changed as an evidence of the spastic nature of the bronchial asthma? Is it not a very plausible conclusion from this experiment that the symptoms of obstruction of the tubes are dependent on a tonic contraction of their muscular fibre? Or is it more probable that these sibilant rhonchi would have disappeared so quickly under the influence of chloral had they depended on a catarrhal tumefaction of the bronchial wall?

I will not at present follow out this question, but I think you will agree with me that the consideration which induced me to make the experiment has been fully justified. It struck me that if this was a spasm of the bronchial tubes, then it ought to relax under the influence of chloral, and the result should be an immediate one; and our expectation has not been disappointed. It appears to me, at least, that the action of the chloral in this case is like the answer to an arithmetical problem; and this induces me to direct your attention more particularly to some controverted points connected with asthma. I shall ask you to glance at the theory of asthma, in order that you may understand my views on the subject.

You may be aware that the older views of the Solidist school, according to which there was a nervous asthma depending on bronchial spasm, have been often opposed during the present century by adherents of the Anatomical school. Authors of considerable note have expressed the view that common asthma is not a nervous affection, but is always connected with emphysema and bronchitis. Even Læennec, who does not deny the existence of a spasmodic asthma, still believes the *catarrhe sec* and emphysema to be the chief causes of asthma. This view naturally gained ground, because, as a matter of fact, patients with emphysema and coughs are most frequently affected with asthma, and no nervous lesion is to be discovered at the autopsy in such cases.

On the other hand, the bronchial spasm has been assigned its proper place in the causation of essential asthma by those authors who have written monographs on the subject, as, for example, Ramadge, Lefèvre, Bergson, Théry, H. Salter, &c.* These authors were practically acquainted with the disease, and concluded, from their study of it, that the cases of essential asthma are not explicable without taking into consideration a spasmodic element.

If some still deny the influence of bronchial spasm on the production of nervous asthmas, as, for example, Wintrich,† we must blame in part the negative or contradictory results of physiological experiment. Nothing certain was formerly known as to the innervation of the bronchial tubes. While, on the one hand, Williams,‡ by irritation of the lungs, and Longet,§ by irritation of the stem of the vagus, said they had produced contraction of the bronchi, Wintrich as well as Budd attained to negative results on repeating their experiments. Rügenberg|| also found that the vagus has no motor influence on the bronchial muscles, but that the increased pressure in the column of the manometer, on irritation of the vagus, is due to contraction of the œsophagus, thus producing pressure on the trachea on the one hand, and drawing up of the stomach, and, through it, of the diaphragm, on the other.

It is, however, noteworthy that it has been established, by the most recent experiments of P. Bert,¶ that Williams was correct in his statements. Bert succeeded, by galvanisation both of the lung and vagus, in producing contractions of the bronchi, and in registering the variations of pressure thus produced by a graphic apparatus. It thus appears no longer doubtful that through the vagus the bronchial muscles may be brought into a state of tonic contraction.

But Wintrich, in his opposition to the theory of the nervous

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* Ramadge, "Asthma," &c. Lond. 1835 ; Lefèvre, "de l'Asthme." Paris 1847 ; Bergson, "Das Krampfhafte Asthma." Nordhausen, 1850 ; Théry, "de l'Asthme." Paris, 1859 ; H. Salter, "On Asthma." Lond. 1860.

† Wintrich, "Virchow's Pathologie," vol. v., part 1. Erlangen, 1854.

‡ Williams, "Report of the Meeting of the British Association, held at Glasgow in 1840." Lond. 1841. "Gaz. Med. de Paris," No. 38, 1841.

§ Longet, "Comptes rendus," T. xv. 1842.

|| Rügenberg, in Haydenhayn's "Studien des physiolog. Instituts zu Breslau," 2 Heft. Leipzig, 1863.

¶ Paul Bert., "Lecon's sur la physiologie comparée de la respiration." Paris, 1870.

nature of the affection, has rested his case not only on the negative results of his irritation of the vagus, but much more on the nature of the physical signs during the asthmatic paroxysm. The depression of the diaphragm, or, otherwise expressed, the distension of the lungs, was a stumbling-block for Wintrich. He could not conceive how an increase in the amount of air in the lungs during the attack could be associated with a contraction of the bronchi, and therefore concluded that a tonic spasm of the diaphragm must be an element in the asthmatic symptoms. The considerations which led Wintrich to this conclusion are, however, much less trustworthy than his clinical observations, as I have shown in another place. ("Bronchial Affections," p. 804.)

The depression of the diaphragm is a constant occurrence in bronchial asthma; it is not, however, the result of a tonic spasm of the diaphragm, but of an increase of the amount of air in the lungs, a blowing-up of the lungs (so to speak), *and this depends on the spastic contraction of the bronchi.*

Apart altogether from the inherent improbability of a tetanus of the diaphragm lasting for hours, it can be shown by direct observation that it does not exist during the asthmatic attack. I have at least convinced myself on every occasion, and you can do the same thing in this case, that the diaphragm presents its usual rhythmic contraction during an asthmatic attack. Its movements are, indeed, less distinct than usual, because its ascent is hindered by the distension of the lungs. And even when no movements of the diaphragm can be detected, this is no proof of a tonic spasm, since the diaphragm may appear immoveable for other reasons.

We cannot directly prove that the depression of the diaphragm and the distension of the lungs are the results of spasm of the bronchi, yet from theoretical considerations we conclude that such is the case. We can show that, given a tonic spasm of the bronchi, the probabilities are, on physiological and clinical grounds, that the action on the mechanism of respiration will actually be what we meet with in asthma.

In order to this, we shall endeavour, in the first place, to explain the physiological significance of the bronchial muscle, and then see how the hypothesis of its tonic contraction coincides with clinical observation.

The bronchial muscles are, doubtless, antagonists of the inspiratory muscles, in the same sense as the muscular fibres of the vessels are antagonists of the heart. They act as factors, in the tonicity of the lung, in opposing the over-distension of the air-passages during inspiration, and they also, along with the elasticity of the lung-tissue, assist in expiration. It is doubtful whether they are actively involved in ordinary expiration, but in forced expiration, and especially in expectoration, they have an expulsive action. In this reference we naturally think of the circular muscular coat of other mucous canals.

We have not as yet been able to prove this function of the bronchial muscle by direct experiment. What we know on this point is the result of deduction and indirect calculation, and, of course, when we have to construct a theory as to the action of bronchial spasm from the general principles of physiology, there is room enough for varieties of individual opinion.

It seems to me that a tonic spasm of the middle and finer bronchi must interfere both with inspiration and expiration. We can easily understand how, from the spasm, the resistance to inspiration, and the negative pressure within the thorax, are increased. There is also no doubt that the inspiratory forces are quite capable of overcoming any obstacles which the narrowed tubes may offer to the entrance of air into the lungs. For the inspiratory muscles can develope a relatively large amount of force. (Donders estimates it at 75 mm. of mercury.)

But it seems to me that the interference with expiration is of greater consequence than the inspiratory disturbance. This is a point which, in the controversy concerning bronchial asthma, has not received sufficient attention, and I shall, therefore, speak more fully of it.

According to my ideas, the spasm of the bronchi is just as inconvenient for the interchange of air in the lungs, as the spasm of the intestine for the proper movement of the fæces. It appears to me, that the bronchial muscles in the spastic state will form a kind of sphincter, which will be more easily overcome during inspiration than expiration, and will interfere with the passage of the air out of the alveoli.

I cannot admit the supposition of Wintrich, that the spasm of the bronchial muscles will necessarily be overcome by the more considerable expiratory forces, such as the elasticity of the lungs and thorax, the expiratory muscles, etc., and this from various considerations.

I cannot see that the most important elements in expiration are really to be called antagonists of the bronchial muscles, the relation which exists between them is rather that of co-operation.

I must call your attention to the fact that the expiratory pressure acts not only on the contents of the alveoli, in which case it might readily overcome the bronchial contraction by *vis a tergo,* but it acts also on the bronchioles themselves. If we suppose the bronchial muscles to be in a state of spasm during expiration, then the affected bronchi exist under the usual expiratory pressure, plus the pressure of the spastic contraction. Is it natural, therefore, to suppose, that under these circumstances the bronchi will be more strongly pressed together than the corresponding alveoli, and that consequently the ventilation of the alveoli will be seriously disturbed? We may even suppose, that as the bronchioles are soft and compressible, they will rather be closed than opened by the expiratory pressure, and this will be almost equivalent to a valve-like closure of the alveoli.

In order to disturb the proper ventilation of the alveoli, we only need, apparently, a small obstruction in the fine bronchi. We see this very well in capillary bronchitis, where closure of the alveoli very readily results from the swelling of the mucous membrane, or accumulation of the secretion. There also, the expiratory pressure is very often not sufficient to remove the obstruction, and the result is, an alveolar collapse where the inspiratory forces are also insufficient to draw air into the alveoli, or a blowing up of the alveoli when the inspiration is still strong enough to suck air into the alveoli, which no longer gets away freely. We must infer the existence of a similar condition in bronchial spasm, as the spastic contraction of the bronchi is certainly not to be estimated at less than the bronchitic obstruction; there arises blowing up of the lung, but no collapse, because the inspiratory forces do their duty, and are strong enough to draw air into the alveoli.

It is also instructive, in relation to this question, to observe how, on post-mortem examination, if any material, such as froth, serum, mucus, or blood exists in the bronchial tubes, the lungs do not collapse on opening the chest, the passage of the air from the alveoli being hindered. This is very striking, as compared with the rapidity with which the lungs of healthy animals fall together on opening the thorax and exposing them to the atmospheric pressure. The difference in these two cases shows very plainly that obstruction of the bronchi, even when it is slight, may counteract the elasticity of the lungs.

I was able to demonstrate to you, a short time ago, a most striking example of acute distension of the lungs in a patient who was suffocated by obstruction of the bronchi with blood. Both lungs were so distended that their anterior edges overlapped. The inflated border of the right lung projected over that of the left 2·5 centimetres, and the triangular space which lies between the diaphragm and the lungs, and which is occupied by the pericardium, was only 4 to 5 centimetres broad and high. This over-distension of the lungs had arisen most acutely during a death struggle, which scarcely lasted a quarter of an hour, from the passage of blood down into the bronchi. Although the lungs were otherwise perfectly normal, they did not collapse in the least when taken from the body, and it required a great amount of force to reduce their volume in the least by pressure; to such a degree did the existence of frothy clot in the tubes prevent the exit of air from the alveoli.

Wintrich and, following him, Bamberger * and J. C. Lehmann,† have laid very great weight on the depression of the diaphragm, as militating against the view of bronchial spasm. They have asserted that in tonic bronchial spasm the diaphragm should be elevated, the circumference of the chest diminished, and the intercostal spaces sucked in—all of which are, of course, just the opposite of what we find in bronchial asthma. I cannot, as I have already mentioned, agree to this view. I would expect to find elevation of the diaphragm, and narrowing of the thorax, and drawing in of the intercostal spaces, if we had to do with spasm of the lung in which both alveoli and bronchi were reduced to a smaller volume. But

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* von Bamberger, "Ueber Asthma nervosum." Würzb. med. Zeitschr., Bd. vi. 1865.

† J. C. Lehmann, "Om Pathogenesen of Asthma hos Voxne." Bibl. f. Laeg. 5, R. xiii. 1866.

such a spasm of the lung does not exist, and in spasm of the bronchi alone the matter is entirely different.

In that case I would expect that the bronchioles would be more strongly contracted than the corresponding alveoli, and that the alveoli, in consequence of insufficient exit for the air, would be acutely inflated, and so the diaphragm depressed. I repeat, once more, that, to my thinking, the antagonists of the bronchial muscles, which are to be sought among the inspiratory muscles, will exert themselves, and draw the air through the narrowed bronchioles into the alveoli, while the air will be slowly and imperfectly expelled in expiration, in spite of the additional force called into requisition.

From the insufficient aëration, and the distension of the lungs, result the most important phenomena of asthma—the air-hunger, and the reflex efforts of the respiratory forces.

The self-regulating power of the respiration, exercised by means of the vagus, has this effect, as shown by Breuer,* that the different kinds of interference with the breathing are combated by suitable modifications of the respiratory process. In cases where the emptying of the lungs is obstructed, then expiration is reflexly strengthened and prolonged, and conversely in imperfect distension of the lungs, the inspiratory act is strengthened. The aëration of the lungs thus corrects itself by a so-called reflex mechanism in the vagus, just like a self-regulating apparatus for ventilation.

Clinical experience accords with this law, only I find that it is not so much the volume of the lungs which is the determining condition as the plus or minus of the interchange of air.

As a matter of fact, we observe at the bedside, that when the entrance of air to the alveoli is imperfectly carried out, then the inspiratory act is always exaggerated, and that when the exit of air from the lungs is hindered, expiration is increased in strength and duration.

The interference with the entrance of air is usually associated with diminution in the volume of the lungs, and it is so far correct to say, that the increased inspiratory activity bears a certain relation to the fulness of the alveoli. But we must not forget that the inspiratory act, by overcoming the elasticity of the thorax and lungs, gives material assistance to expiration, and that, therefore, inspiration is also increased as an auxiliary, even in obstructions to expiration. This auxiliary exaggeration of inspiration may, therefore, be observed, even with distension of the alveoli.

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* Breuer, "Die Selbststeuerung der Athmung durch den N. vagus," Sitzungsber. der k. k. Akadamie der Wissensch. zu Wien. Bd. lviii. Abth. ii, Nov. 1868.

On the other hand, we can say in respect to exaggerated expiration, that it is associated with increase of the volume of the lungs; for in all cases in which we observe any considerable interference with the passage of air from the alveoli—whether this depends on bronchitic swelling of the mucous membrane, or on bronchial spasm, or on emphysematous diminution of the elasticity of the lungs—there is always a certain amount of over-distension of the lungs with depression of the diaphragm.

The prolongation and exaggerated intensity of expiration is very marked in bronchial asthma, and this corresponds with the supposition of a bronchial spasm; for expiratory dyspnœa is as characteristic of narrowing of the smaller bronchial branches, as inspiratory dyspnœa is of stenosis of the larger air-passages.

It will be convenient in future to distinguish two principal forms of dyspnœa—that which is chiefly inspiratory, and that which is chiefly expiratory. You may take as a type of inspiratory dyspnœa, the respiration in croup, œdema glottidis, obstruction of the trachea, etc.; and the best example of the chiefly expiratory form is to be found in bronchial asthma. If you compare the type of respiration, in obstruction of the larynx or trachea, with that of an asthmatic attack, the difference will be sufficiently plain. We shall make the comparison now, and first observe the respiration in croup.

In croup the patient uses all the accessory inspiratory forces. The muscles of the throat, neck, and thorax are all in full activity, in order to raise and widen the chest. The head and neck are extended; the larynx follows the movements of the hyoid muscles, which draw it downwards at every inspiration. The ribs mostly move upwards. The diaphragm also acts strongly; does not, however, pass downwards sufficiently, but rather draws in the xiphoid and lower ribs (7–9). The respirations appear relatively slow, because the inspiratory phase occupies a longer time. In spite of all his exertions, the patient does not succeed in drawing in the proper quantity of air, and the result is a marked rarefication of the air in the lungs, which in a high degree favours the falling in of yielding parts of the thorax during inspiration. The supra-sternal region, the supra-clavicular region, but especially the lower part of the sternum and the neighbouring cartilages of the ribs, are sucked in during inspiration. A well-marked transverse depression arises, from this dragging in of the thorax, immediately above the situation of the costal insertion of the diaphragm, showing that the reserve space of the pleural sac is not filled up by the lungs, in spite of the exaggerated inspiration. During expiration the sucked-in cartilages hasten back to position, the depressions in the neck disappear, the larynx returns to its normal situation, and the air passes quickly out. Expiration generally occurs easily and shortly, if no complication (especially in the finer air-passages) leads to other conditions.

Very different from this is the *type of respiration in the attack of bronchial asthma.* We find here, indeed, generally a distinct exaggeration of the inspiratory act; but this is only of subsidiary importance. The alteration is mainly in expiration, which occurs with the use of all the forces, and appears as much prolonged as inspiration in croup.

The duration of expiration is generally double that of inspiration, and the prolongation of the respiration, which is generally more marked in asthma than croup, is chiefly dependent on the prolonged expiratory phase. It is not unusual to find the patient occupying four or five seconds in each expiration, in order to empty the lungs, while inspiration is finished in one or two seconds. The pause between expiration and inspiration is altogether absent.

All the accessory muscles are brought into use in expiration, and act most vigorously. This is most distinctly seen in the abdominal muscles, although those of the back are also concerned. The recti abdominis are tightly stretched, the upper regions of the abdomen remain full, while a transverse depression above the umbilicus indicates that the transversus muscle meets with an obstruction in its upper half, formed by the abdominal organs, which are unable to ascend sufficiently, on account of the distension of the lungs. The transversus also draws the lower ribs a little in towards the vertebræ, so that the sterno-vertebral diameter may appear reduced. In other respects there is no drawing in of the thorax observable.

Speaking and coughing are difficult, on account of the insufficient expiration. It therefore happens that the patients are almost entirely unwilling or unable to speak during the paroxysm, and that in order to the expectoration of a thin sero-mucus, which is secreted after a time, there occur paroxysms of coughing in which ten to fifteen expiratory impulses may be counted to one inspiration.

On auscultation, we observe that the respiration is full of râles. Inspiration and expiration are sibilant, but especially the latter. The sonorous, dry rhonchi of the prolonged expiration are very characteristic, as they indicate the constriction of the bronchi. They may be heard even in those cases where the asthma has occurred as a nervous affection without preceding bronchitis. We can determine the degree of the bronchial spasm from the sibilant râles. In slight spasm the sibilant rhonchi in inspiration and expiration are almost equally loud; in stronger spasm they are stronger in expiration, but when the bronchial spasm has reached its highest degree, they disappear completely, because the interchange of air is in most places reduced to a minimum, and the conditions are absent for the production of any respiratory murmur in the finer air-passages. For the most part we hear a short and weak whistle in inspiration, and a longer and stronger in expiration. The vesicular respiratory murmur is in all circumstances concealed or absent. I think you will find that all this corresponds with the theory of bronchial spasm which I have brought before you.

If you further avail yourselves of percussion, you will find that it also proves the existence of the distension of the lungs produced by the bronchial spasm, often after a few inspirations, as well as the diminution of the movements of the diaphragm associated therewith.

In the lower regions of the thorax, especially behind and at the side, you will hear a modification of the tympanitic note, which I am accustomed to call the bandbox note, from its resemblance to the percussion note of a bandbox. The bandbox note is sonorous and deep, but has a somewhat tympanitic timbre. The physical conditions of this modification I shall not attempt at present to explain, but I can assure you that they depend on an increased tension of the alveolar tissue. The bandbox note is to be heard wherever a considerable portion of over-distended lung lies close to the thorax (whether the alveoli are acutely distended or permanently emphysematous), and we can demonstrate on the dead body that this modification depends on the distension of the alveoli by artificially distending the lung to a certain extent, and then percussing.

You will also prove by percussion that the borders of the lung are depressed for one to three intercostal spaces, and do not alter in inspiration and expiration, but again pass upwards gradually after the asthmatic attack is over.

To summarise the chief objective signs of bronchial asthma, we recognise—(1) the exaggeration of the inspiratory and expiratory efforts, especially the marked increase in strength and duration of expiration; (2) the sibilant stenotic respiratory murmur in the finer bronchi, which, however, in the highest degree of the attack, may pass into a cessation of all auscultatory phenomena; (3) the bandbox percussion-note corresponding to the distended portion of lung; (4) the increase in area of the lung as determined by percussion, and its uniformity in inspiration and expiration; (5) the limited movements of the depressed diaphragm.

All these phenomena are easily brought into harmony on the supposition of a spastic contraction of the bronchi, with consequent distension of the lungs. On the other hand, it would be impossible to explain the sibilant rhonchi, which distinctly point to a narrowing of the air-tubes by a tonic spasm of the diaphragm.

The tonic spasm of the diaphragm has been studied by Duchenne* in animals; he faradised the phrenic, and thereby produced contractions of the diaphragm. But the phenomena produced do not, in my opinion, correspond with those of bronchial asthma. Duchenne says, for instance, that the respiration consists of a sharp inspiration and a short expiration, while the lower half of the thorax is permanently dilated. He also brings forward an observation of Valette, the only one of tonic spasm of the diaphragm in man of which he knows. In this observation, however, the expiratory straining of the abdominal muscles was absent, the upper parts of the thorax showed very limited inspiratory and expiratory movements, the number of the respirations was 46 in the minute, and in every part of the chest intense bronchial breathing (?!) was heard. These are results which, if the observation is correct, harmonise as little as may be with the phenomena of bronchial asthma. But, besides this, Duchenne asserts that contraction of the diaphragm leads in a few minutes to commencing asphyxia, and it is therefore improbable, on this ground also, that we have a diaphragmatic spasm in bronchial asthma.

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* Duchenne, "Bulletin de l'Acad.," 1853, T. xviii.; et "Electrisation *localisée," p. 488.* Paris, 1855.

Is there a form of asthma dependent on diaphragmatic spasm? To this I would answer, that tonic spasm of the diaphragm occurs in tetanus, and leads to symptoms of asphyxia, but without the features of asthma, and that clonic spasm of the diaphragm has been occasionally observed, and forms a peculiar variety of asthma, which is not to be identified with bronchial asthma.

I observed an interesting case of this kind in the year 1861, and will give a sketch of it here, for the sake of the differential diagnosis.

The patient was a girl, 22 years of age, who was placed under my care on account of capillary bronchitis, and distension of the lung. She suffered at first from ordinary dyspnœa, but later on she had paroxyms of a peculiar, very violent asthma, recurring every evening. The attacks were in the form of spasmodic tetanic inspirations, and forcible expirations. After a short spasmodic inspiration, in which all the inspiratory muscles were concerned, the thorax remained for some seconds (three to four) in the inspiratory position, with the muscles of the neck stretched, and the epigastrium prominent. During this period a deep transverse depression formed in the mesogastrium; the neck began to be distended, the expiratory muscles of the abdomen came into action, but no emptying of the lungs followed. Only when the tetanic inspiration relaxed did the expiration occur quickly and audibly, as in sobbing. Immediately after the rapid expiration the short inspiration again occurred, and the fixation of the dilated thorax, and so on. The impulse of the heart was not to be detected in its usual situation at the height of the inspiration, but in the scrobiculus cordis, and we could feel the heart distinctly passing upwards during expiration. When the spasm passed off, the impulse was felt in the fifth intercostal space, between the sternum and the line of the nipple. The heart-sounds were weak but distinct, the pulse during the spasm small and regular (110–120), and, after the attack had passed off, 84–88. On auscultation there was no vesicular murmur, but a dull inspiratory and rough expiratory sound. Swallowing was impossible during the attack, but not speaking; the movements of the glottis were not at all impaired. The patient complained of severe pains in the region of the diaphragm, especially on the left side, and conducted herself in a very excited manner. If chloroform was administered, which was often done during the attack, then a state of quiet occurred after a few inhalations; but as soon as the narcosis had passed off to some extent, the spasm always returned, till it passed off in from a quarter of an hour to an hour.

The spasmodic attacks recurred almost daily for two months, at first with great regularity, afterwards irregularly. Their intensity increased, and once or twice they were so violent that six to twelve seconds were counted during the tetanic fixation of the thorax. The inspiratory spasm was frequently combined with violent spasmodic fits of coughing, which took the place of the forced expirations. Consequent on these fearful attacks, emphysema and bronchial ectasis developed, and these led to the death of the patient four years after. When once the emphysema had developed, the patient had also, in addition to these attacks, a permanent dyspnœa, of the ordinary asthmatic type (prolonged whistling expiration, etc.). We could then easily enough distinguish between the expiratory dyspnœa and the spasm of the diaphragm.

There can be no doubt that we had here an instance of reflex clonic spasm of the diaphragm; we could, indeed, feel the spasmodic movements of the diaphragm. The whole features of the attack reminded one of a well-known form of spasm of the diaphragm, singultus, and it was only distinguished from that, in respect that the inspiratory spasm could be observed with great completeness, and lasted several seconds. I have since then observed two similar cases of spasm of the diaphragm in hysterical patients, but they were never so violent or persistent.

I must also relate to you another case of inspiratory asthma which came under observation a year ago in a hysterical patient, because it throws some light on the controversy concerning asthma.

In this case the affection takes origin in the larynx; it belongs, therefore, to the category of the so-called laryngeal asthma, if you choose so to reckon it, but does not depend on a spasm of the glottis, but paralysis of the dilators of the glottis (the musculi crico-arytænoidei postici).

The patient is a lady, 33 years of age, belonging to a respectable family; she possesses a robust frame and a good constitution, but has been troubled for some years with violent hysterical attacks. When she came under my observation there was a paralysis of the right fore-arm, with very painful contraction of the hand, which has now disappeared, and in addition to this a peculiar disturbance of the respiration, which still persists. The patient never breathes normally, but there are quiet periods in which the type of respiration differs little from the normal, in spite of the paralysis of the vocal cords. These are interrupted by more or less violent attacks of dyspnœa, which are painful to see.

In the quiet periods the patient breathes with about double the normal frequency, but without special effort. The muscles of the neck and intercostals are only slightly active, and the diaphragm acts as usual. In conversation the sentences are frequently interrupted by short pauses, during which the patient draws breath. Occasionally one hears a slight shuffling râle in the larynx in inspiration and expiration. Any exertion increases the respiratory movements.

On laryngoscopic examination the movements of the cords are seen to be inverted. The arytenoid cartilages, with the vocal cords, do not move apart in inspiration, and do not approach in expiration. The voice, which was formerly aphonic, is now better, but the right cord does not during vocalisation perfectly accord with the left. In other respects both cords go together during vocalisation, and then go apart at the succeeding inspiration (normal type of movement).

From time to time the respiration becomes sighing and more difficult. Each respiration is then accompanied by a shuffling sound, and their frequency is increased (24–40); the action of the intercostal and cervical muscles is also increased. The larynx is moved forwards, and the two vocal cords approach during inspiration till the glottis is reduced to a small slit. The diaphragm tries to depress itself still further, but its movements are small, because the patient makes no regular expirations. The expiration is not exaggerated; it appears as if the patient had no need of strong expiration.

On auscultation we hear in every part of the lungs only weak respiration, and over the larynx gasping stridulous râles.

After respirations of this kind have existed for several days (only interrupted by sleep), the patient appears very tired, and complains of pain in the throat, and in the neighbourhood of the insertion of the diaphragm.

But the form of respiration described often increases till violent attacks develope, which are so remarkable that I cannot avoid describing them to you.

In these attacks the chief phenomenon is an almost perfect closure of the glottis, which is produced by the vocal cords during inspiration laying themselves together, so as to render the entrance of air impossible. The glottis opens distinctly in expiration, but closes immediately on inspiration. We can see this with the laryngeal mirror. During the paralytic closure of the glottis, which is often of long duration, there occurs a complete cessation of respiration, with great subjective feeling of want of breath. The cessation of respiration is not absolute, however, for although the thorax hardly moves at all, still there are visible movements of the diaphragm, which serve to a certain extent to aërate the lungs.

The attacks occur often without any known cause, or they come on from excitement, or pain, or after the administration of chloroform in order to apply a plaster-of-Paris bandage. In the great majority of cases they occur in the evening, rarely in the course of the day or night. Their duration varies from half an hour to five hours. The patient feels when attacks are coming on, and those about her can see it on her. She appears sleepy and dull, and the change of the quiet respirations into the loud and stenotic, is the indication of the occurrence of an attack.

The patient now breathes deeper and deeper; loud inspiratory and weak expiratory laryngeal sounds are heard, as if of sucking; the patient speaks little, and the voice is weaker, the vocal cords approach continually closer in inspiration; the muscles of inspiration come into fullest activity, those of the neck and larynx being especially on the stretch. The larynx is pulled strongly forwards and somewhat upwards. The inspiratory phase becomes continually longer, and the expiratory remains short and weak. The respirations often occur only four to six times in the minute. The shuffling, laryngeal sound passes more and more into a sharply resounding modification, for which it is difficult to give a proper designation. It might be called an inspiratory cry, and it is occasionally so loud as to be heard in a neighbouring room, or on the street.

When the height of the attack is reached, there occurs a prolonged closure of the glottis, which resembles an actual cessation of respiration. The patient sits upright in bed, apparently apnoic, generally with the head backwards. The muscles of the neck and hyoid bone are mostly on the stretch. The larynx is fixed, and carried forwards and upwards by the firmly contracted supra- and infra-hyoid muscles. We see, among others, the genio-hyoidei, sterno-hyoidei, sterno-thyreoidei and omo-hyoidei projecting as tense, fleshy bundles. The sterno-mastoids, scaleni, and trapezius are also firmly contracted. The upper part of the thorax seems to be pulled up, and the depressions of the neck have sunk in. Slight movements of the intercostal muscles and the diaphragm are visible; the abdominal muscles are passive. No râles are to be heard, and there is no cough. If she coughs accidentally or by request, the glottis opens to the expiratory current; this is easily proved by laryngoscopic examination. But the cough is weak. Swallowing is disturbed. If we give her, for instance, chloral, she gulps it down, and then coughs or sneezes.

After the patient has been in this condition for 20, 30, 40, 60, or even 100 seconds, the picture changes. She becomes restless; a slight lividity spreads over the countenance; the face is painfully distorted; movements of the jaws are made; she moves the body hither and thither, touches surrounding objects nervously with the hands, calls on the nurse, hangs on her neck, clutches the bed-cover, presses spasmodically on any neighbouring object, supports herself on one arm and sends the other in the most various directions; spastic contractions fly over the face, through one or both arms, through the whole body—it is a horrible sight for those unaccustomed to it!

At last the fit resolves, generally with a hoarse cry; the glottis is once more opened, and the thorax sinks together in expiration. The patient breathes distinctly; we can hear the inspiratory whistle and the short expiratory sound. Then begins anew the onset of prolonged inspirations. The respiration of the patient is again characterised by the forward movement of the larynx, and the spasmodic action of the accessory muscles of inspiration with limited movement of the diaphragm, till again the closure of the glottis occurs, and a cessation of respiration for 20, 30, or more seconds.

It may thus for hours alternate between simple obstructed inspiration and almost complete cessation of respiration. By degrees the breathing becomes quieter and less prolonged, and passes into the tempo of our patient's ordinary respiration. There remains only great languor, and a painful feeling of tiredness. The chest, the larynx, and the diaphragm are painful. This also passes off, and the patient enjoys her "good times," which, however, seldom last longer than a few days. The longest interval was three weeks. This unusual disease has existed now since September, 1864. In spite of this, the patient has suffered very little in her general condition and nutrition, and no organic affection has developed, except a slight pulmonary emphysema. A very chronic inflammation in the knee, which has again become worse during the last few months, has, of course, no connection with the paralysis of the vocal cords, yet it has tended to aggravate the apnoic attacks, on account of the pain, and the manipulations necessary to apply a gypsum bandage.

The diagnosis of this uncommon form of respiratory disturbance admits of no doubt, from the repeated laryngoscopic examinations. It might also be reached by the way of exclusion; bronchial asthma, spasm of the diaphragm, and paralysis of the diaphragm may be at once excluded, and the only difficulty is the exclusion of spasm of the glottis, which presents great similarity to the attacks of our patient. Yet, if we consider that at the commencement of the disease the respiratory affection was associated with prolonged paralytic aphonia, and that our patient, besides the proper paroxysms, exhibited, for days, well marked stridulous inspiration, which could not occur with spasm of the glottis, then we are able to diagnose a paralytic laryngeal asthma without the aid of the laryngoscope. We are not astonished at the similarity of the apnoic attacks to those occurring in spasm of the glottis, because in both cases the actual disturbance is caused by an inspiratory closure of the glottis, and the effect will be the same, whether the arytænoidei proprii cause closure of the glottis by spasmodic contraction, or on account of paralysis of their antagonists. It is, perhaps, similar to what we see in paralysis of the limbs, where we so often find the flexors predominating over their antagonists, the extensors, to such an extent as sometimes to lead to permanent contractions.

I believe, therefore, that we have here a case of that rarer form of paralysis of the vocal cords which first received due consideration from C. Gerhardt,* namely, paralysis of the dilators of the glottis. At first, when the patient was aphonic, the closers of the glottis must have been similarly affected; but this has disappeared under electric treatment, till now there is only occasionally a trace of hoarseness, and whispering voice.

Let us now return from this consideration of inspiratory forms of asthma to bronchial asthma, concerning which I have still some observations to make. We have not yet spoken of the conditions and causes of asthma, and I must therefore bring under your notice some points in *Etiology* and *Pathogenesis*.

Bronchial asthma may occur, as you know, as an independent neurosis, but is much more frequently met with in connection with congestion and catarrh of the respiratory mucous membrane.

The nerve which produces the bronchial spasm seems, according to our present knowledge, to be the vagus. We have, at least, no sufficient evidence that the bronchial muscles derive their nerves from the sympathetic; although, from analogy with smooth muscle elsewhere, we should not be astonished to find the bronchial muscles furnished with fibres from the sympathetic.

The irritation of the bronchial nerves may occur either directly (from the medulla oblongata, or the peripheral stems), or, more frequently, by reflex action.

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* C. Gerhardt, "Studien und Beobachtungen über Stimmbandlähmung." Virchow's Archiv, 27 Bd., 1863.

I know of no undoubted case of *direct* production of bronchial spasm from the central nervous system; but some cases of bronchial asthma seemed to owe their origin to swollen tracheo-bronchial glands. If this supposition be correct, then we may suppose that there was here a direct irritation of branches of the vagus by the varying state of swelling of the enlarged lymphatic glands.

The *indirect reflex* form may have its origin in very different parts of the body. At least, we see bronchial asthma occurring not only in connection with diseases of the respiratory organs, but also under the influence of affections of the circulatory apparatus, the abdominal organs, and the constitution as a whole. The attacks of idiopathic bronchial spasm produced in otherwise healthy persons by mental emotions, impressions on the senses, idiosyncrasies, etc., are to be set down as reflex. The conditions and course of the reflex action are, however, obscure, and do not allow of even a hypothetic explanation.

At the same time it must be said that, in my belief, the centripetal lines of communication of the respiratory mucous membrane are most frequently involved in the origin of bronchial spasm. I count among these the nerves of smell, which play an important part in cases of idiosyncratic asthma. When an otherwise healthy man is attacked, as we often see, by asthma, as often as he smells certain flowers, strong perfumes, tobacco-smoke, the steam from the kitchen, metallic or animal emanations, and such like, then the nerves of smell must have been involved in the reflex action. Similar is it in other cases of asthma, which are brought on by the inhalation of dust, pulverised substances, or even irritating vapours; in these the sensory branches of the respiratory mucous membrane have conveyed the irritation to the motor roots of the vagus.

Those peculiar cases of catarrhal asthma which are produced by the smell of dried hay, grasses, flowering rye-grass, etc., show that signs of congestion of the respiratory tract precede the asthmatic symptoms, and go parallel to them. The substances smelt produce not merely the sensation of smell, but also a sympathetic disturbance of the circulation, which manifests itself as a catarrhal congestion of the mucous membrane of the nose, eyes, larynx, and the other air-passages. The roseola and urticaria which arise after the use of strawberries, crabs, mussels, etc., are examples of a similar kind of sympathy proceeding from the nerves of taste.

The congestive element plays at least a part in bronchial asthma; for not only the idiosyncratic cases, where the attacks are induced by the inhalation of odours and dust, but also the other forms frequently begin with nasal and bronchial catarrh. There are persons who have an attack of asthma whenever they have a catarrh.

The connection between the hyperæmia and the bronchial spasm is unfortunately undetermined.

We may suppose, *à priori*, that either the bronchial congestion causes the bronchial spasm, or that the hyperæmia and spasm are the conjoint effects of an irritation of the centripetal nerves—are, in fact, both brought about reflexly.

The frequent occurrence of asthma in bronchitis and emphysema seems to be in favour of a causal connection between the congestion and spasm. On the other hand, we occasionally see attacks of bronchial asthma which undoubtedly begin without any catarrhal symptoms, but which end with catarrhal symptoms, giving the impression as if the bronchial hyperæmia were a result of the asthma. Trousseau* lays considerable weight on this fact, and we can state in confirmation that a seromucous bronchial secretion is not infrequently produced by the attack indicating a bronchial hyperæmia. Indeed, we sometimes find symptoms of bronchial and pulmonary œdema developed during the attack.

It is to be remarked, however, that it is not easy to recognise a congestion of the bronchial mucous membrane so long as it remains dry, and we cannot, at least, distinguish it by physical examination from the symptoms of a simultaneous asthma. It is possible, therefore, that even in those cases where the asthmatic attack appears to begin without catarrh, a congestive turgescence of the mucous membrane of the finer bronchi may be the first occurrence, to be followed, at the height of the attack, by an increased secretion or not, as the case may be.

If this were the case (but we do not know if it is so), then there would be a correspondence in this respect between catarrhal and idiopathic asthma. But the question would still remain, whether bronchial hyperæmia can of itself produce bronchial asthma, and in what way it may do so?

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* Trousseau, "Clinique Medicale de l'Hôtel Dieu de Paris," T. i., 1861.

I have to confess that I have not been able to come to any distinct opinion on this point. I have already, in my work on "Bronchial Affections," expressed the opinion that the narrowing of the lumen of the bronchi, as a result of hyperæmic swelling, leads to insufficient evacuation of the alveolar air, and an exaggeration of the expiratory effort, and that this may, perhaps, lead to a sympathetic contraction of the bronchi. According to this view, I supposed that the hyperæmic turgidity of the bronchioles, by disturbing the respiratory pressure, leads, in a kind of mechanical way, to a spasm of the bronchial muscles; and, so far as the cases connected with bronchitis and emphysema are concerned, I still hold it. I am convinced that every capillary bronchitis produces either atalectasis, when the inspiratory forces are too weak, or over-inflation when the inspiratory forces are strong enough. This inflation of the lungs, I believe, produces reflex expiratory exaggeration, both in emphysema and in recent capillary bronchitis, and this, in the majority of cases, passes into bronchial spasm.

In the dilatation of the alveoli in emphysema, there is brought about a state of toleration; but in the altered state of the circulation, it requires a very slight accident to induce a fresh bronchial congestion, and we may thus explain the frequent recurrence of bronchial catarrh and the expiratory dyspnœa which results from an increase of the inflation of the lungs, and so readily passes into asthma. These phenomena will, in recent idiopathic capillary bronchitis, depend on the degree and extent of the hyperæmic smelling, and according to this also will be the amoumt of expiratory disturbance.

Whether this hypothetical explanation which I have constructed, and which also harmonises well with the facts determined experimentally by Breuer (l.c.), applies also to idiopathic bronchial asthma, I am not yet sure. It is possible that bronchial spasm may be directly reflected from other parts of the body. We are not assisted by the clinical signs, especially because the bronchial spasm leads to distension of the lungs equally with capillary bronchitis, and even more rapidly.

Allow me now, in conclusion, to summarise the most important points in a few short sentences.

I think I have shown you that:

1. Bronchial asthma must depend on a tonic spasm of the bronchial muscles.

2, This is demonstrable by means of clinical observation (type of respiration, obstructive râles in the fine bronchi, and rapid disappearance of the symptoms after the use of chloral hydrate.)

3. The bronchial spasm leads to acute over-inflation of the lungs and depression of the diaphragm.

4. This acute pulmonary inflation, which is to be distinguished from permanent alveolar dilatation (emphysema), arises whenever the fine bronchial branches are narrowed (whether by spasm or bronchitis), and the inspiratory force is not too weak.

5. The ventilation of the alveoli is easily disturbed by local obstructions of the bronchioles, and in that case the air more readily passes in during inspiration than out during expiration.

6. In bronchial spasm there is a valve-like closure of the tubes by the spastic muscle, and this is less easily overcome by expiration than inspiration.

7. The distension of the lungs may be detected by a modification of the tympanitic percussion-note (the band-box note).

8. The depression of the diaphragm is no sufficient ground for the supposition of a tonic spasm of the diaphragm.

9. The symptoms of spasm of the diaphragm are different from those of bronchial asthma.

10. Tetanus of the diaphragm cannot exist for several hours, for it leads more quickly to fatal asphyxia.

11. Clonic spasm of the diaphragm produces symptoms like those of singultus, in which the inspiration is much more disturbed than expiration.

12. In bronchial asthma, the obstruction and exaggeration of expiration are more essential than those of inspiration.

13. The expiratory dyspnœa is as characteristic of narrowing of the smaller air-passages as inspiratory dyspnœa of stenosis of the larger passages.

14. This corresponds with the law of the self-regulation of respiration through the vagus, according to which any insuffi-

ciency in the filling of the lungs increases inspiration, and any hindrance to the emptying of them increases expiration.

15. Croup and laryngeal asthma serve as prototypes of preponderatingly inspiratory dyspnœa.

16. Bronchial asthma occurs mostly by indirect, reflex irritation of the vagus.

17. We can distinguish a catarrhal and an idiopathic bronchial asthma.

18. In both forms, congestive phenomena occur in the respiratory mucous membrane.

19. There are cases in which the attack is not preceded by any symptoms of alteration of the bronchial mucous membrane.

20. In bronchitis and emphysema, the bronchial spasm may be produced by a disturbance of the respiratory pressure (distension of the lungs), and, in this case, it would correspond with an inefficient co-operation of the expiratory forces, which are reflexly exaggerated.

CLINICAL STUDIES

OF THE VARIOUS FORMS OF

CHRONIC DIFFUSE NEPHRITIS.*

BY

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OF KIEL.

GENTLEMEN,—You will have remarked that amongst the sick under our care on the medical side of this hospital there are always some who are passing albuminous urine. Richard Bright, an Englishman, was the first who, by his writings, more than forty years ago, permanently drew the attention of physicians to this symptom. The consequence of this was that at first many physicians called every case of albumenuria Bright's Disease. But careful observers soon learnt that albumen might pass through the kidneys under very various conditions, always as a pathological phenomenon certainly, but that simple albumenuria ought by no means always to be regarded as a symptom of settled kidney disease, not even when dropsy was added to it. For this reason we exclude from our idea of Bright's Disease all those cases of albumenuria which are the result of severe valvular disease, muscular degeneration of the heart, or disturbances of the lesser circulation leading to general cyanosis; although the effect of damming back the venous circulation may make itself apparent in the intimate structure of the kidneys of such patients. Neither are those cases of transitory albumenuria which make their appearance in every sharp feverish attack of any duration included under this term. It is doubtful, to say the least, whether this febrile albumenuria does, in any case, arise from an anatomical change in the tissues of the kidney. I shall, on another occasion, give my reasons for believing that it is merely a symptom of fever, and that it is the result of the effect of overheated blood upon the nerves of the vessels. Furthermore, we do not talk of Bright's Disease when the albumenuria is the effect of the violent action of poisons within the system. Finally, all authors are agreed that the amyloid degeneration of the secretory vessels, which so very frequently gives rise to albumenuria, dropsy, and the sequelæ of permanent cachexia, is, *per se*, a condition totally foreign to our idea of Bright's Disease. Nevertheless, this amyloid degeneration is very often complicated with the anatomical changes characteristic of Bright's Disease.

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* **Abstracts of several Clinical Lectures delivered at Kiel.**

Having now told you what is not Bright's Disease, you will be anxious to learn from me what it really is; in other words, what the anatomical changes in the tissues of the kidney are, which ought to be considered characteristic of the disease. We will hear, first of all, what the authorities on the subject have to say. Even clinical students, after some experience with albumenuria, feel it necessary to denote the event by an expression which shall describe the anatomical changes, instead of by a mere name. It is quite incorrect for a pathological anatomist to think he can still talk of the undiminished authority which the elastic idea Morbus Brightii ought, in accordance with its meaning, to enjoy in clinical diagnosis. But I think this pathological anatomist is quite correct when he explains that the pathological anatomy of nephritis has been more studied, but still is less complete, than any portion of his investigations, and when he says that hitherto nephritis has defied our attempts to describe it anatomically in different stages. But we practitioners cannot wait until pathological anatomy has reached this stage. Clinical experience of diseases of the kidney teaches us that certain symptoms correspond to certain anatomical changes, and that in this manner we can safely diagnose the post-mortem appearances from the symptoms during life. Various well characterised descriptions of the disease, corresponding to well-marked changes in the kidney have been got by this means. And with this success we must, for the present, be content; for, as yet, clinical experience by no means authorises us to reckon these as the different stages of one definite disease.

Even the authors of the new German Handbooks and Compendiums of Clinical Medicine have not borne this in mind. Instead of finding "Bright's Disease," or the synonym of this once naturalised term, as the title of the dissertations on this subject, we now find "Parenchymatous," "Interstitial," or even "Diffuse Nephritis"; in other words, in place of the "Elastic Idea," Morbus Brightii, which in no way denoted the anatomical changes in the kidney, a term is now employed which suggests a diffusely spread process of inflammation through the tissues of the kidney. In their anatomical descriptions, the authors describe three consecutive stages—Inflammatory Hyperæmia, Exudation, and Atrophy. No doubt post-mortem appearances exactly corresponding to these various stages do occur; but it is another question whether these various appearances ought really to be given out as the various grades of development, or stages, of one and the same disease. In my opinion, this view of the pathological anatomy has hitherto failed. There are many good pathologists who are decidedly opposed to the unity of these various pathological conditions of the kidney. Amongst these was my late colleague, Colberg.

We will see, first of all, what clinical experience teaches us, and what correspondence there is between it and the usual account of the post-mortem appearances. If you look into the clinical handbooks and works on this subject, you will be surprised to find that there is either a total absence of all clinical details of the disease, or that there is a very short description of them, without any attempt to show a correspondence between the clinical symptoms and the supposed anatomical stages of Bright's Disease. The symptoms which may occur during the varied pathological changes of the kidney are enumerated, and perhaps arranged to show how frequently this or that symptom appears in a certain number of cases, but not one of the authors has taken the pains to show that one group of symptoms is connected with any of these three stages. But no experienced and careful clinical teacher will tell you that Bright's Disease regularly begins with the symptoms which are said to belong to the first stage, nor that the symptoms of the second stage are the necessary, or even the regular, forerunners of the symptoms of contracting kidney (Bright's granular kidney), described by Traube in so masterly a manner. In spite of Traube's excellent treatise, I have repeatedly seen the most able physicians mistake the symptoms occurring in this so-called third stage of Bright's Disease—kidney contraction, I call it—and thus incorrectly diagnose the kidney mischief, owing to their having trusted to the succession of these three stages, and to the consistent presumption that the corresponding symptoms must appear in similar order at the bed-side. It is the knowledge of this fact which has induced me to discuss our subject of to-day.

Impartial observation at the bed-side immediately compels us to separate the *acute* from *chronic* diffuse nephritis. And it is quite certain that acute diffuse nephritis is nothing more than the first stage of Bright's Disease, as usually described; though I am perfectly aware that in making this assertion I deny the entire first stage of a great number of chronic cases, in consequence of the separation mentioned above. But facts will speak for me and justify this decision.

The kidneys of persons who have suffered from scarlet fever, diphtheria, a severe chill, or more rarely from measles or small-pox accompanied by albumenuria and dropsy, very often exhibit the anatomical changes characteristic of the so-called first stage of Bright's Disease. But, as you know, the great majority of these recover within a short space of time, even if the urine has been reduced to a very small quantity and has had a considerable quantity of blood mixed with it for some weeks. A minority only die of *acute* nephritis. It is quite exceptional for one of these cases to become chronic and to end in death after some years. Amongst the numerous cases in my experience, I have seen but one in which albumenuria, the result of scarlet fever, continued for eighteen months: even then complete recovery followed.

On the other hand, an investigation of the *chronic* cases shows us that their onset is very rarely indicated by really acute symptoms, as is the case with the acute form of the disease. I have only seen one case; the patient got an unmistakeable chill from having fallen asleep, half undressed, before an open window in the winter, after having danced all night; and the commencement of kidney mischief was shown by fever and backache. General dropsy very soon followed, and lasted nearly a whole year. The albumenuria continued uninterruptedly for three years, when death followed from pericarditis. In the majority of cases, the most careful inquiry yields no information of the commencement of the disease; it has developed chronically from the beginning; it has, as the patient often tells us, crept on. This is especially the case where the symptoms terminate in contraction of the kidney. Such persons not unfrequently say that they were never ill before, and yet, after death, we find their kidneys in an advanced stage of degeneration. But I expressly add, that, in very exceptional cases, diffuse nephritis may begin acutely, and become chronic afterwards.

In order now to point out to you the clinical symptoms characteristic of the *two further stages* of diffuse nephritis or Morbus Brightii, and to show in what way these two stages are to be clinically distinguished from one another, I may be permitted to describe their usual symptoms separately.

We will take the so-called *second* stage first, but leave out of consideration all those cases of chronic nephritis which have associated themselves with wasting disease. As has been said, we then find that these cases rarely relate to persons who have partially recovered from an acute febrile attack, or to such as have felt constantly unwell since a drenching or thorough chill, but more frequently to those who have suffered intermittently for a longer time, and most frequently to such as have noticed for some time an unaccountable, but marked diminution of their strength and weight, accompanied by pallor and shortness of breath. With these symptoms œdema of the hands and feet are associated. In the greater number of cases it is the appearance of the œdema which first causes the frightened patient to seek medical advice. They seldom complain of any striking symptoms in connection with the urinary organs; there is no complaint of tenderness on pressure over the kidneys, nor of troublesome desire to urinate. It is only by personal investigation that we can find any tenderness in the renal region, or learn that, though urination has been unusually frequent, the total amount of water passed is very small. This frequency of urination deceives many patients with regard to the amount of urine they pass in the twenty-four hours; they think that because they evacuate so frequently they pass a great deal. And for this reason I cannot too strongly urge you to ascertain personally the amount of urine in every case of kidney disease. The urine passed by these patients in such sparing quantity has a cloudy appearance, is throughout of high specific gravity, has usually a decidedly acid reaction, and, when heated, throws down a solid coagulum of albumen. The whole liquid turns into a stiff jelly before the boiling point is reached. In the copious dirty sediment which sinks to the bottom of the urine soon after its evacuation, microscopical examination shows numerous cylindrical casts, some of which are hyaline, some dark and opaque with granular matter, the greater number are of considerable diameter, the rest are more slender, and usually hyaline. Here and there on these casts we find bodies resembling the nuclei of the epithelium of the uriniferous tubes, very rarely uninjured epithelial cells. Red blood-corpuscles are hardly ever present, whilst bodies resembling white blood-corpuscles are, on the contrary, to be found regularly, and sometimes in considerable quantity.

The œdema when once established often increases rapidly, and very commonly reaches an excessive degree. The fluid does not limit itself to the subcutaneous cellular tissue, but invades also the lungs and the serous cavities of the body. Not unfrequently it is the direct or indirect cause of death; for it may lead to the sloughing of some very important portion of skin—that of the scrotum, for instance—and death may follow from septicœmia; or an accumulation of fluid in the cavity of the thorax, or in the parenchyma of the lungs, may suffocate the patient. In those cases only in which the patient comes under treatment immediately on the commencement of dropsy, and in which we are able to call forth a copious secretion of urine, have I ever seen a rapid disappearance of the dropsy, and a gradual diminution of the albumen, followed by the commencement of complete convalescence. But in hospital practice the dropsy has usually existed some months before any treatment is begun, and then it may be a long time before we can get the mastery of it: in such cases, but temporary recovery may ensue. An increased flow of pale urine of low specific gravity, and containing less albumen, announces the complete disappearance of the œdema, and then, for the first time, the great emaciation of the patient is apparent. These cases take a long time to recover their strength, weight, and good looks. I have seen some rare instances amongst these who have returned to their business for years, though their urine continually contained a small amount of albumen, and a copious quantity of thick fibrinous casts containing oil-globules. But none of them recovered their full strength; all remained emaciated to a certain extent, and had a sickly appearance. Their skins were mostly dry and covered with epidermic scales, whilst their hair was thin and bristly. Amongst all the cases belonging to this group which I have watched for a course of years up to their death, I have only met with three in which there was not a relapse of dropsy; of these three, two died of intercurrent inflammatory diseases—the one of pneumonia, the other of pericarditis; the third died with symptoms of uræmia. In the majority of these cases a diminution of the urinary secretion occurs sooner or later, and dropsy returns to disappear no more. The most frequent immediate causes of death are erysipalatous inflammation of the skin with purulent infiltration of the subcutaneous tissues, serous or purulent exudation into the pleural or peritoneal cavities, and œdematous or purulent infiltration of extensive portions of pneumonic tissue. I have rarely seen patients belonging to this group get an attack of uræmic convulsions whilst the dropsy was on the increase. But when great quantities of water have been suddenly got rid of by any other channel than the kidneys, for instance, by copious artificially-induced diarrhœa or sweatings, so that a sudden and wholesale reabsorption of the dropsical fluid has been brought about, then I have frequently seen uræmic convulsions make their appearance.

In these cases, which end fatally in the first attack of dropsy, the post-mortem appearances correspond to those usually described under the *second* stage of Bright's Disease. The kidneys are *enlarged* far beyond their normal size. The cortical substance is chiefly affected by this enlargement, and, from its pale yellow colour, contrasts strikingly with the hyperæmic pyramids. If one of these cases has dragged on for years, there may be an entire absence of œdema, and the kidneys may be reduced to their normal size and weight, or even to less than the normal. And when this is the case, the surface of the kidney is uneven, and coarsely granular. The elevations are of a yellowish colour, and separated from one another by callous bands of connective tissue. The cortical substance, which is chiefly affected by this wasting of the organ, has an anæmic yellow colour, and feels somewhat firm. Not unfrequently the diminution does not fall upon the two kidneys equally, sometimes not upon every portion of the same kidney. Now and then we find one-half of a kidney distinctly reduced below the normal, whilst the other half has preserved its abnormally large size.

If we now compare the symptoms which appear when death occurs in the second stage of Bright's Disease with those which appear in the *third* stage from *true* contraction of the kidney, we shall be surprised to find how many patients belonging to the latter group preserve an appearance of almost complete health up to the time of death. Every one of those amongst you who goes into general medical practice will find that amongst the patients who have consulted you for a short time on account of some trivial disorder—such as a headache, a slight giddiness, a palpitation or pressure at the heart, the so-called rheumatic pains here and there in the body, but most frequently on account of repeated dyspeptic symptoms, with or without vomiting—there will be some cases of sudden death, and that this may occur even with those who have noticed no loss of appetite or strength, and have never felt sufficiently ill to consult you. This sudden death is sometimes preceded by the symptoms of cerebral apoplexy, sometimes by a series of epileptiform convulsions following close upon one another, and of dreadful violence. If epileptiform convulsions have been the immediate cause of death, their outbreak is usually preceded for a short time by a change in the patient's health, or he has previously undergone some unusual exertion; for example, we may find that the patient gets ill for no tangible reason; that he is attacked by violent sickness, diarrhœa, general malaise, or some intercurrent febrile attack; or that he had undertaken some unusual and extreme exertion, or had been exposed to violent emotions. It is very seldom that these attacks of convulsions occur without precursory symptoms, without any of the foregoing warnings of an outbreak. The apoplectic form, on the contrary, occurs, as a rule, in the midst of business or pleasure, without any warning, except perhaps an occasional attack of palpitation of *the heart,* oppression, or giddiness.

In these cases we usually find the body well nourished and the muscles well-developed; there is not a trace of fluid in the subcutaneous tissues nor in the serous sacs; all the organs of the body, with the exception of the heart and kidneys, are quite healthy; and only when an attack of apoplexy has terminated life do we find anything wrong with the brain. I will describe the post-mortem appearances in detail later on, and in the meantime I will lay before you the symptoms of contracted kidney, as they occur when they are not suddenly cut short by apoplexy or uræmic convulsions.

Such a sudden and unexpected termination of life from contraction of the kidney by no means frequently occurs, in fact, very rarely; death, as a rule, occurs after a tedious illness, the symptoms of which vary very much in the several cases. Palpitations of the heart, oppressive feelings, headache—in the shape of migraine, as Traube frequently observes—and giddiness, are the most common symptoms; symptoms which naturally result from the hypertrophy of the left ventricle of the heart and the consequent increased tension in the aortic system. In every case of hypertrophy of the left ventricle, for the origin of which you cannot find satisfactory valvular disease, you should bear in mind the possibility of the enlargement of the heart being due to kidney disease. It is very seldom that such patients, of their own accord, complain of any abnormality with regard to their urinary secretion. So long as they are pretty strong, they not only pass water very frequently but also very abundantly, much more so than healthy men would do on a similar diet. It follows, as a matter of course, that this increased secretion of urine has to be counterbalanced by a more copious drinking of liquids. I have been consulted by a patient who, alarmed by his unnatural thirst, suspected diabetes mellitus, whilst he was suffering from contraction of the kidney. Under these circumstances, the daily amount of urine may be very much in excess of the normal. One of my patients evacuated over 6,000cc. in twelve hours. But I have also been astonished to find how very frequently those patients who do not suffer from true polyuria are disturbed in the night to pass their water. A Mr. N. never wanted to pass his water during his hours of business, from 9 a.m. to 4 p.m., whereas he was obliged to get up three or four times in the night to empty his bladder. The urine is clear and very pale, of a yellowish green colour, and very seldom lets fall any distinct sediment. The specific gravity of the urine is subject to certain variations, according to the daily quantity of the liquid; but after the kidney disease has well advanced it always remains low, and never reaches the specific gravity of the urine from a healthy man on the usual diet, even under conditions which bring about important changes in the urine, such as a profuse loss of fluid through the bowel or skin. When I speak of the specific gravity of urine, let me be understood to speak of the specific gravity of the mixed quantities of the twenty-four hours' secretion. The reaction of this pale, light urine is generally slightly acid, perhaps neutral, and sometimes, without any interference on our part, it may continue slightly alkaline. As a rule, the urine from contracted kidneys contains albumen. I have met with but one case where the urine temporarily contained no albumen. Others have observed this occur more often. But under all conditions the quantity of albumen, reckoned by its percentage, remains small, and becomes more insignificant, as the changes in the kidneys advance and the specific gravity of the urine sinks. Sometimes only after renewed search we succeed in discovering in the very sparing sediment isolated, generally slender, hyaline casts of fibrin, to which, here and there, uninjured epithelial-cells from the uriniferous tubes are sticking. Red blood-corpuscles may also be present; in one case that I saw, they were present in such quantity that for several days the urine threw down a considerable sediment of blood.

There is no dropsy during the whole course of more than half of these cases of contracted kidney. But anasarca appears before death when the secreting surface of the kidney has been reduced to a very small amount. The dropsies of these patients are often of a very erratic nature; they change their position in a remarkable manner, or disappear altogether for a time, and hardly ever give rise to those enormous swellings of the whole body which are so characteristic of the so-called second stage of Bright's Disease. Moreover, dropsy may appear during the earlier stage of this contraction when some intercurrent disease of the thoracic organs—endocarditis or pleural exudations, for example—has brought about a sudden change of arterial tension and of urinary excretion. As these disturbances of the circulation are removed or compensated the dropsy disappears completely. As a rule, dropsy is not permanent until the patient gets very weak.

The debility of the patient is still further increased by interruptions of digestion. Obstinate dyspepsia, distaste for every kind of solid food, frequent and persistent retching—the only result of the last being a slimy, watery fluid, for the patient can take no nourishment at these times—are very common symptoms in these cases: their consequences are general emaciation, anæmia, and a sickly cachectic appearance.

Those peculiar affections of the retina which impair the sight so grievously, and which are known as Bright's Retinitis, often occur in cases of contracted kidney.

As I have already remarked, when the patient becomes weaker he passes less water, and, after this, he may never again pass the normal quantity. In addition to this the density of the urine gets very low, lower even than in an earlier period of the disease, when there is an abundant amount of urine. So that the solid urinary ingredients are, proportionally, even more reduced than the watery excretion. In this way a disproportion arises between the production and the excretion of the solid urinary ingredients. And, as a rule, death approaches, in one way or the other, with more or less certainty whenever these effete matters are retained in the fluids and tissues of the body.

When, from the experience of a large number of cases, you are able to ascertain the immediate causes of death in this disease, you will find that a large, perhaps the larger proportion died from inflammatory attacks which associated themselves with the original disease; and that when death occurs in this way it is generally the result of inflammatory exudations into the pericardium or pleura, or of extensive inflammatory infiltrations of pulmonary tissue which exhibit a tendency to suppuration; but sometimes the disease may be cut short by extensive peritonitis or phlegmonous erysipelas of the skin and subcutaneous tissues. There must be some reason for this unmistakeable tendency to inflammatory affections of the most varied portions of the body in this disease, and I should, with Traube, be inclined to attribute it to the poisoning of the fluids and tissues with the excretory products which ought to have been carried off by the kidneys. One can easily conceive that the accumulation of this filth in the tissues must have a most poisonous effect upon them, and that under the influence of this any trivial irritation might set up acute inflammation, which we must consequently attribute to uræmic blood-poisoning.

And this is undoubtedly the correct interpretation of that series of symptoms which precede death in a large number of these cases of advanced renal disease, and which have been very properly designated symptoms of chronic uræmia. The persistent vomiting which torments these patients, even when they do not take any nourishment, must be looked upon as one of these symptoms. That extremely troublesome itching which brings the patient to despair, and makes him tear his skin by scratching, is also a symptom of uræmia. Uræmic amaurosis, according to my experience, seldom occurs without some decided change in the organ of sight; and I have observed it with the greatest care. This symptom appears and disappears very suddenly. Also spasms of isolated muscles and of groups of muscles appear in these cases of chronic uræmia; they may last for weeks, and then may be followed by an outbreak of epileptiform convulsions, terminating suddenly in death. But an increasing stupidity, gradually deepening into torpor and complete coma—the immediate precursor of death—is the commonest nervous symptom in chronic uræmia. That these symptoms are really connected with the retention of urinary matter in the fluids and tissues of the body no one will question who has observed, as I have in two cases, that even the skin of the patient for some days before death may be covered with a crystalline excretion of urinary products.

Having now given you an account of the symptoms of this disease, I will describe shortly the essential and characteristic post-mortem appearances, omitting the occasional and accidental changes. The diminution of both organs is the most striking change I have to mention to you. The loss in size, as a rule, affects both kidneys pretty equally, and in advanced cases it may be so considerable that the kidneys may not be larger than a child's, and their weight not more than half what it ought to be. They are throughout remarkably tough and firm. The capsule, now dense and thick, adheres so firmly to the outer layer of the organ that when it is forcibly torn away portions of the kidney come away with it. When the capsule is removed the exterior of the organ is found to be covered with pretty equally-shaped knobs—granulations—which vary in size from a pin's-head to a hemp-seed, and are thickly crowded together. Similar granulations are sometimes to be found on the larger knobs. The colour of these kidneys is generally a dark brownish red, but I have seen contracted kidneys remarkbly pale and anæmic. An examination of such kidneys in section shows us at once that the contraction has resulted in a loss of cortical substance, of which oftentimes only a thin band surrounds the reduced and closely crowded pyramids. In the remains of the cortical substance we may frequently find numerous cysts, varying in size from a millet- to a hemp-seed, and filled with clear, transparent contents. The pelvis of the kidney is not infrequently enlarged to a pouch of considerable size. Besides these changes in the urinary organs, you will have to record, as a regular and essential condition, the hypertrophy of the left ventricle, the cavity of which you will find enlarged and its muscular walls considerably thickened. The endocardium may be cloudy here and there, and perhaps thickened; whilst the valves, on the contrary, except very rarely when it must be considered accidental, exhibit no change, at least none that in any way interrupts their proper functions.

Judging from the post-mortem appearances, there cannot be the slightest doubt that these changes in the kidney must be the result of a morbid action which has been going on for a long time, oftentimes for years. And yet the majority of the patients treated by me for this disease, and whose whole life-history I could trace, had never once suffered from dropsy.

After several of his countrymen had expressed their objection to grouping together under one head the so-called second stage of Bright's Disease and contraction of the kidney, Johnson, in 1859, led by similar experiences to my own, came to the conclusion that the theory of the identity of the changes in the kidney, until now comprehended under the name of Bright's Disease, was no longer tenable, and that it was inconsistent with both clinical experience and anatomical appearances. For if the disease is identical in every case of so-called Bright's Disease, the changes found in the kidney ought to differ from one another only according to the varying intensity of the disease and the stage to which it has arrived. From a clinical point of view he specially pointed out the difference between the two foregoing diseases with regard to the frequency with which dropsy accompanies them. In 26 cases of chronic *enlargement* of the kidney, without complications which could have given rise to dropsy, he found dropsy 24 times (92 per cent.); and in many cases it was intense, general, and lasted for a long time. In 33 cases of *contracted* kidney he found, on the contrary, only 14 cases (42 per cent.) of dropsy; which were mostly slight, with the œdema in isolated positions. "If every case of contracted kidney," says he, "has passed through that inflammatory enlargement, it is difficult to understand how it happens that the majority of those patients who live on to the final stage of kidney degeneration altogether escape the dropsy which attacks nearly all patients, in a greater or less degree, who die in the supposed earlier stage of the same disease." He comes to the conclusion that the large swelled kidney always remains large and does not contract, and that, on the other hand, the contracted kidney has never been swelled.

With regard to the more minute changes which the kidneys undergo during the two morbid processes in question, Johnson says that the *contracted* kidney is caused by a primary disintegration of the epithelium lining the urinary canals; in consequence of which it is loosened from the walls of the canals and is washed away with the urine, leaving the urinary canals quite empty. In the *large swelled* kidney, on the contrary, the epithelium generally remains sticking to the walls of the canals, undergoing meanwhile certain changes, from a light granular cloudiness to a fully developed fatty degeneration, until at last it is entirely replaced by an albuminous or fibrinous matter, which more or less fills up and stops the urinary canals. Johnson says nothing about the changes in the *interstitial tissue* in these two forms of kidney disease. He believes that the difference in the disturbances of the functional activity of the kidneys may be due to the different changes which the epithelium undergoes.

The shedding of the epithelium by the contracted kidney ought theoretically, according to him, to hinder the passage of the solid urinary products and to promote that of the liquid. On the other hand, the obstruction of the urinary canals with broken-up epithelium and fibrinous casts ought to impede the passage of water through the large swelled kidney. But these deductions are not sufficiently confirmed.

Meanwhile, it by no means escaped Johnson that, considered both clinically and anatomically, there were apparently transitionary forms between the two kinds of chronic nephritis described by him. But from the apparently exceptional cases he got most important evidence in favour of his view with regard to the essential difference between these two forms of kidney disease. As I have before told you, we meet with patients who have suffered for a year or longer with intense general dropsy, and who, in the first onset of the disease, secreted very small quantities of a very concentrated urine loaded with albumen, but who now pass copious quantities of urine of a very low specific gravity, and containing a very small amount of albumen. The nature of the urine resembles that from the genuinely contracted kidney in every particular, except that we constantly find a copious deposit of shining casts, some of which glitter like wax, and some of which are cloudy with granular matter. Such persons, I say, may at the same time gain weight and strength—not their full strength, but they may nevertheless live for years in the exercise of their calling, despite a continued albumenuria. If such a patient dies at last of an inflammatory disease, or of uræmia, one finds the kidneys contracted of course, but the appearance of such *secondarily* contracted kidneys is, according to Johnson, so strikingly different from that of kidneys wasted by *genuine* contraction, that it is hardly possible to mistake one for the other, or even to question that the contraction in each case is caused by a series of essentially different pathological changes. The main points of difference that Johnson finds are—that the kidney, swelled by chronic enlargement, is always pale and anæmic, even when a supplementary contraction has taken place, whilst the genuinely contracted kidney, even in the most extreme degree of contraction, is often comparatively red and hyperæmic. But he especially points out that the chronic enlargement of the kidney only shows a slight tendency to atrophy and contraction; for experience shows that, as a rule, the kidneys remain enlarged even after the disease has existed for years.

Traube was the first amongst German writers who described the clinical symptoms accompanying contracted kidney. He admits various processes as causes of the contraction; but, whilst feeling strongly that the term Bright's Disease ought to be abandoned because it includes within it various morbid processes, he explains the anatomical changes brought about by these morbid processes quite differently to Johnson. Traube more particularly disputes a primary (inflammatory) change of the epithelium in the various forms of chronic nephritis. He says the cloudiness and fatty degeneration of this cellular structure result from inflammatory action in the interstitial tissue. Traube's view has met with opposition even in Germany. My late colleague, Colberg, after many years' careful study of the pathologico-histological changes of the kidneys, in the main agreed with Johnson, and particularly pointed out, as characteristic distinctions between the genuinely contracted kidney and that wasted by secondary atrophy, that in the former the band of epithelium lining the still intact uriniferous tubes was found wholly unchanged, whilst the epithelial cells in the yellow granulations of the latter were generally found in a state of fatty degeneration.

Amongst more recent writers, Grainger-Stewart, in his treatise on Bright's Diseases of the Kidney, which appeared in 1868, says most positively, that what has hitherto been designated the second stage of Morbus Brightii is an inflammatory process, in which the Malpighian corpuscles, the uriniferous tubules, and their epithelium, all partake at first, and from which a fatty degeneration of the epithelial cells follows. As the *débris* of the epithelium is removed by absorption, or carried off by the urine, a diminution of the whole organ follows, and if a microscopical examination is now made, the interstitial tissue is found to be relatively increased, because so many uriniferous canals are completely emptied and have disappeared. But uriniferous canals filled and distended with a dark fatty mass still remain in the cortical layer of the atrophied kidney. This writer asserts that the genuinely contracted kidney is, on the contrary, the result of a primary hypertrophy of the interstitial tissue, which sets up an atrophy of the whole organ. There ought not, therefore (according to his theory), to be any accumulation of fatty matter in the uriniferous tubules. He contends that this process has not an inflammatory character; and because he considers that this change is analogous to cirrhosis of the liver, he proposes to call this form of contracted kidney the cirrhotic kidney.

For my own part, I do not feel competent to offer an independent opinion with regard to the foregoing anatomical changes in the kidneys whilst there is such a diversity of opinion as to the connection of events. We must wait for further pathologico-anatomical investigations to fill up the gaps in our knowledge in this department and bring to an end these contradictory statements. It only remains for me to-day to draw your attention to the variety of the foregoing morbid processes and their eminent importance in clinical practice.

If we could satisfactorily show the causes which give rise to the two different forms of the foregoing chronic kidney affections, it would be of great pathological interest. But in many cases hitherto, as I have already indicated, every attempt in this direction has failed. Still, the little we do know with regard to the causes of one or the other of these diseases most decidedly indicates an original difference between them.

As I have before stated, *chronic inflammatory enlargement* of the kidney may, in isolated cases, originate from acute nephritis following a severe chill or scarlet fever. But in my experience malaria is the most frequent amongst the well-established causes of this disease. I have never seen this chronic kidney affection follow albumenuria occurring during any other acute febrile attack. That not only the lardaceous, but also the chronically inflamed large kidney, may, in many cases, be found after death when the patient has suffered from a truly wasting disease accompanied by persistent fever, I have told you before.

I must speak much less positively with regard to the ætiology of the *truly contracted kidney.* This disease always begins imperceptibly, and has existed in a latent form a long time before its detection, and in almost all my cases the patients could only form a conjecture as to the cause of their trouble, beyond the general evils to which every man is more or less subject. I have never seen this chronic affection follow an acute attack, nor could I ever make out any connection between it and persistent attacks of intermittent fever. As a rule, I have searched in vain through the histories of my patients for something which might be considered the real cause of their illness. Now and then habitual abuse of spirits and excessive beer drinking might be taken as a cause, but in proportion to the prevalence of such habits this effect certainly followed but seldom. In my private practice I get few habitual drinkers amongst my cases of kidney disease, which is certainly not the fact with the patients in this hospital. I may mention that one of my patients was accustomed to drink enormous quantities of strong tea. That contraction of the kidney frequently results from gout is especially pointed out by most English writers, and more especially of late by Grainger-Stewart. In this country we have but little opportunity for the study of gout or its sequelæ. But from my slight experience in this department I think I can confirm the statements of the English who see so much more gout than we do. Lastly, I have seen a case in which chronic lead-poisoning accompanied by colic and palsy of the extensors of the fore-arm (the result of snuff-taking), preceded the symptoms of contracted kidney. Again, the connection between chronic lead-poisoning and this kidney disease, which was formerly asserted but afterwards denied, is now confirmed by later English writers.

Now that it has been pointed out, and I think on sufficient grounds, that the chronic inflammatory kidney enlargement cannot be considered as a previous stage of the true contracted kidney, and that the two conditions of the kidney must be considered the result of two totally different morbid actions, it is necessary that I should point out to you the effect of the foregoing anatomical changes upon the physiological functions of the kidneys, and the connection between the essential symptoms and the disturbances of the physiological functions. I shall be obliged to limit myself to a short statement of facts owing to the limited time at our disposal, and shall reserve for another occasion a more complete discussion of the following questions.

But I must first of all remind you that a twofold duty is allotted to the kidneys; they have not only the task of removing from the blood the specific urinary ingredients, *i.e.* the refuse resulting from the metamorphosis of the used-up tissues, but they also serve as regulators of the amount of water in the serum of the blood; for they withdraw from the circulation surplus fluid which has been taken up during the assimilation of a too dilute emulsion of chyle. If this latter duty of the kidneys be not sufficiently performed, more water will be left in the blood than corresponds to the natural quantity, and the serum becomes watery; moreover, owing to the increased amount of fluid within the vessels, their tension must increase unless it be counterbalanced by a copious transudation of water from the blood into the tissues and serous sacs of the body, and by a lessened absorption of fluid such as necessarily accompanies hydrœmia. Increased transudation of water from the blood and diminished re-absorption give rise to dropsy.

If we now compare the daily quantity of urine, or, in other words, the daily quantity of water which passes through *chronically inflamed* kidneys with that which passes through *genuinely contracted* kidneys, we shall see that the swelled kidneys secrete an abnormally small quantity of water, whilst the true contracted kidneys very often secrete an abnormally large quantity. In the former disease the daily average quantity for weeks or months may not exceed 400cc. to 500cc.; in the latter the daily quantities are often from 3,000cc. to 4,000cc. Here we find a full explanation of the facts that dropsy almost always—always, in my experience—at some time or other accompanies chronic kidney enlargement, and that it is absent from the majority of cases of genuine contraction. As I have said, the dropsy accompanying chronic enlargement not infrequently disappears for ever when a full flow of urine has been secured by appropriate treatment. And, on the contrary, the quantity of urine from a case of genuine contraction may sink below the average and be followed by hydrœmia and dropsy, if the kidney substance withers away to too great an extent, or if, from some cause or other, the mechanical power of the heart sinks below a certain point.

These further questions now force themselves upon us—why do the inflammatorily enlarged kidneys, as a rule, secrete so little urine? and why do the genuinely contracted kidneys so often secrete an unnaturally large quantity? In answering these questions, we cannot take into consideration the amount of water in the blood, though, in a healthy condition, it exerts so striking an influence over the urinary secretion, because we know by experience that patients belonging to the former group, and who secrete so little urine, are supplied with blood-serum which is unnaturally loaded with water, whilst we are by no means entitled to consider that this is the case with the patients belonging to the second group, who pass such large quantities of water. Amongst the remaining factors coming into consideration in connection with the secretion of the urine, the extent of surface of the secreting vessels, or, in other words, the number of the still intact Malpighian corpuscles, should perhaps be mentioned first; next, with what rapidity and under what pressure the blood passes along the secreting vessels of the kidney; then, the diffusibility of the fluid on its way along the uriniferous tubes; then, the condition of the vessels, through the walls of which the secretion passes; lastly, the obstruction which the urine meets with on its way down the urinary canals owing to accumulation of fibrin casts. All these circumstances have to be taken into consideration, not only with regard to the quantity of the urinary secretion, but also with regard to its quality.

If we make a microscopical examination of a genuinely contracted kidney, we invariably find that a great portion of its filtering apparatus—the Malpighian corpuscles—has got compressed by surrounding masses of connective tissue, and has become altogether impervious to the blood-current, whilst the urinary canals in connection with them have degenerated and are deprived of their epithelial coat. By this means the circulation through a large part of the kidney is cut off. Consequently blood will flow with greater rapidity and under higher pressure through the still pervious Malpighian corpuscles. This increased resistance to the circulation through the kidneys does not fail to produce an effect upon the entire aortic system, of which the increased tension exhibits itself in the full pulse and the hypertrophy of the left ventricle; the latter of which Traube has convincingly shown to be the effect of the obstruction and the means of compensation thereof.

If we now bear in mind the effect of these circumstances upon the urinary secretion, it will be obvious that as long as the hypertrophied heart acts energetically, there will be, proportionally, an unnaturally large quantity of water forced through the pores of the still intact Malpighian corpuscles, in consequence of the pressure within the vessels being so much increased. This filtrate is carried along the urinary tubes with more than usual swiftness, and there is consequently but little time for concentration to take place within them by means of diffusion. So it may happen that, despite the important diminution in the secretory vessels, a greater quantity of urine may pass through contracted kidneys than through sound kidneys under natural circumstances. The quantity of urine passed by one of my patients was measured seventy-six times in the course of six months, and it was found that he had passed on an average 2,200cc. every day. The secretion of such kidneys is always more watery than that of sound ones, though the two may be alike in other respects. But if the number of the still intact Malpighian corpuscles should sink below a certain point in consequence of the advance of contraction; or if, for some reason or other, the otherwise abnormally increased pressure within these vessels should diminish, the daily quantity of urine secreted by the contracted kidneys may get unnaturally small, and, as a consequence thereof, hydrœmia and dropsy may appear. I must very especially draw your attention to the dependence of the quantity of urine upon the conditions of the heart's innervation. It is especially in this disease of the kidneys that the daily amount of the urine is subject to the greatest fluctuations and exhibits the most striking changes from day to day. One of my patients, who, on the 2nd December, 1869, passed but 500cc. of urine, of 1,018 sp. gr., secreted on the 5th December, 7,500cc., of 1,008 sp. gr. In this case emotions had undeniable influence over the power of the heart, and thence over the quantity of the urine. On this change in the energy of the heart's action also depends without doubt the fluctuation of the œdema, together with the coming and going of the same in so many cases of contracted kidney.

Fewer trustworthy means are afforded us for understanding why the secretion of the inflammatorily enlarged kidneys is diminished. If the patient has already got into a highly anæmic condition, arterial pressure is sure to be unnaturally diminished, whence follows less secretion through the kidneys. In addition, the pale anæmic condition in which we invariably find such kidneys post-mortem, shows us that, owing to the swelling of the entire parenchyma, the movement of the blood through them must have been interrupted and rendered slower. And physiological experiments on such secretory organs as permit control in this respect have taught us that the rapidity of the circulation and the amount of secretion stand in direct relation with one another. And again, physiological experiment and clinical observation show that this fact also holds good for the kidneys; so that, owing to the slowness of the circulation in the swelled anæmic kidneys, the rapidity of the secretion through them must be diminished. To what extent changes in the filters, *i.e.* in the walls of the vessels forming the Malpighian corpuscles, which have been observed by some writers, amongst others by Colberg, may influence the rate of secretion, I must leave undecided. But it is quite certain that a slowly secreted urine on its way through the urinary canals of the kidneys loses by diffusion a great portion of its water, and in this way may get concentrated to such a degree that its specific gravity exceeds that of the blood-serum. In one of my cases the specific gravity of a highly albuminous urine, judged from the total daily quantity, reached 1,041, another even to 1,042. Finally, and unquestionably, there is another factor to be taken into consideration with regard to the retention of urine from chronic inflammatory enlargement of the kidney, and that is the stoppage of the urinary tubes by means of fibrinous casts, which one often finds in very great quantity in the kidneys when removed from the body. If you will bear in mind what an extremely slight pressure applied to the ureter is sufficient, according to Hermann's researches, to keep back the entire secretion from the kidneys of a dog, you will certainly not be inclined to put a slight value upon the effect which the stoppage of the numerous uriniferous tubes with fibrin casts would have upon the urinary secretion in man. Also the daily quantities of urine passed, not only by different individuals, but also by the same person during the course of chronic inflammatory enlargement of the kidney, are subject to considerable variation. But the average quantity of many days' secretion is always under the normal. Thus, the average daily quantity in one of my cases during six weeks was under 500cc. In another, the daily average during five months, estimated from ninety-four measurings, amounted to nearly 600cc.; later, during seventeen months, and estimated from two hundred and seventeen measurings, it was 970cc.

I have already told you, that when the inflammatory swelling of the kidney goes on to secondary contraction, the daily quantity of urine increases, and that the urine then, with regard to its chemical composition, exactly resembles that from the genuinely contracted kidneys, and that when this occurs, the dropsy which formerly existed may altogether disappear. To this I have now to add, that should such patients recover their strength to a tolerable extent, hypertrophy of the left ventricle makes its appearance. I am convinced of this both by clinical observation and by post-mortem examination. Nevertheless, I have never found such an excessively increased urinary secretion as is frequently the case with genuinely contracted kidneys. The last-mentioned patient, who, towards the latter end of his life again came under my care, and whose urine, with regard to chemical composition, exactly resembled that from contracted kidneys, passed 1,430cc. every day on an average, reckoned from eighty measurings; the greatest daily quantity during this period was 2,375cc.; the least, 700cc.

The retention of the specific urinary ingredients in the blood of patients suffering from the foregoing forms of disease is, owing to its so often proving fatal in the shape of uræmia, of no less practical importance than that of the amount of fluid evacuated. This is not the place to enter into an argument as to what the real nature of uræmia is. Since Voit's experiments, I consider these controversies have been definitely settled, and confess I held the view that it is essentially an accumulation of urinary matter in the fluids and tissues of the body which ought to be termed uræmia amongst patients suffering from kidney disease, and that in many of these cases the chief effect is due to the urea, whether it be direct, or, in rare cases, indirect, after the conversion of this material into carbonate of ammonia, and that this urea produces an effect which, for the most part, varies in proportion to its quantity. What the effect of the remaining urinary constituents may be in these cases we know but very little for certain. That, on the contrary, the accumulation of urea in the body may go on to such an extent that it may, even in lifetime, be recognised on the skin as a crystalline excretion, I have already pointed out. Also there appears to be a decided difference between these two forms of kidney disease with regard to the frequency with which uræmic symptoms appear in their course. Uræmia is a much more frequent symptom of the contracted kidney than it is of chronically enlarged, although the former, as a rule, secretes so much urine, whilst the latter, on the contrary, secretes so little. This fact is nevertheless but partially explained when we examine the urine alone; we must bear in mind the other circumstances which come into relation with it in order to clear up this point.

If we compare the results of the urinary analyses in these two forms of kidney disease, we shall find a much higher *percentage* of urinary matters in the sparingly secreted urine of the swollen kidney than in the more copious but more watery urine of the contracted kidney. In the urine secreted by a patient suffering from most severe dropsy, and belonging to the first group, and whose daily quantity was 395cc., with sp. gr. 1,041, I found 4·9 per cent. of urinary ingredients, whilst the lightest urine from contracted kidneys that I have examined only contained 0·6 per cent. of urinary ingredients. But, as is the case in the healthy, the percentage amount of urinary matter in the urine of these patients varies with the quantity of water passed in a given time. The more copious the secretion the lower the density and the percentage amount of urinary matter. If one succeeds in urging the inflammatorily enlarged kidney to more copious secretion, the percentage of urinary matter in the urine falls. The same patient who, in August, 1868, furnished me with urine amounting to 395cc. daily, and containing 4·9 per cent. of urinary ingredients, secreted, two months later, a daily quantity of 1,500cc., of 1,009 sp. gr., and containing 0·9 per cent. of urinary ingredients; and the patient suffering from kidney contraction, whose greatest daily quantity of 4,200cc., with 1,006 sp. gr., had only contained 0·6 per cent. of urinary matter, passed on another day only 2,100cc., of 1,015 sp. gr., and containing 2·1 per cent. of urinary ingredients.

We can draw no conclusion as to the total amount of urinary ingredients from the percentage alone. In some cases certainly, the greatest total amounts of daily ingredients correspond to the highest percentage; in others this is not the case. In order to obtain a knowledge of the total amount, it is necessary therefore to collect and measure carefully the total daily secretion, and, after the determination of the percentage of the solid urinary ingredients of a portion of this, we must find by calculation the total daily secretion of urinary matters. The examination of the results of such measurings and analyses collected together for many days enables us to form an opinion with regard to the secretory performance of the kidneys. And it also shows us how changeable the daily quantities are in both the foregoing forms of kidney disease. For example, one robust, well-nourished man afflicted with contraction of the kidney secreted in one day, with 4,200cc. of urine, of 1,006 sp. gr., and 0·6 per cent. solid ingredients, 25·2 grammes; on one of the following days he secreted, with 2,680cc. of urine, of 1,015 sp. gr., and 1·9 per cent. solid matter, 50·92 grammes; and on an average of 76 analyses during six months, he passed 33 grammes of solid urinary ingredients every day. One man who died of chronic inflammatory enlargement of the kidney passed, in January, 1869, according to the results of sixteen analyses made in the course of six weeks, 20·43 grammes daily. One other patient belonging to this group, on an average of fifty analyses made during the course of five months, passed 16·5 grammes daily; later on, from an average of one hundred and ninety analyses, during a course of observation extending over seventeen months, he passed 20·6 grammes daily.

The examples given appear to indicate that contracted kidneys, owing to the polyuria accompanying them, secrete a more copious daily quantity of solid urinary ingredients than the inflammatorily enlarged kidneys. And this is correct for many cases, at least for a certain time during the course of the disease. Such patients indeed, as the example which has been given shows, under favourable circumstances sometimes evacuate more solid urinary matter than less well nourished though healthy persons. But should the daily amount of urine, from any of the before mentioned causes, become diminished, the daily secretion of urinary ingredients may be reduced to a very small amount; for, as I have already said, the density of the urine from contracted kidneys does not increase much when the amount of urine has been very considerably reduced. That girl

K., who has been under treatment for months on account of contracted kidneys, was attacked, in March, 1868, with fresh endocarditis, accompanied by high fever. A month later she died with uræmic convulsions. During this month she evacuated every day, on an average of twenty analyses, 763cc. of urine, and only 12·7 grammes of urinary ingredients. A captain of a ship, who got an exudation into the pleura in addition to contraction of the kidney, passed, during the latter days of his life, on an average, 1,120cc. of urine every day, which contained 15·9 grammes of solid urinary ingredients. I have already pointed out that in many cases the retention of urea in the fluids and tissues of the body is a safely established cause of uræmia. Therefore in these cases a disproportion between the production of this material and its excretion from the body must have preceded the uræmic symptoms. It would be of great practical interest if we could tell when such a disproportion between production and excretion began. No doubt we are able to ascertain by means of careful analyses how much urinary matter is passed each day out of the body by the ordinary route through the kidneys. But as yet we possess absolutely no means of obtaining a knowledge of the quantity of urinary matter *formed* within the body in a given space of time, nor have we any means of ascertaining what amount is removed from the blood by other means than that of the kidneys, *e.g.* by the dropsical effusions into the serous sacs of the body. If a similar quantity of urinary matter were always produced in the human body, a glance at the tables of the results of carefully executed analyses would immediately inform us of the danger of an impending uræmia. Indeed, in two of my patients who died of uræmia and whose urine had been collected and analysed for a long time, the daily secretion of the solid urinary ingredients slowly sank for a month before death until it reached 12·7 grammes in the one case, and 11·75 grammes in the other. But a very low rate of daily secretion of solids suffices, under certain circumstances, for such a removal of this dangerous product of metamorphosis from the fluids and tissues of the human body as is consistent with life. A young woman, 28 years old, pregnant for the first time, suffered with perpetual vomiting, and died, finally, of pneumonia; her urine was analysed twenty-five times; she secreted but 8·84 grammes of solids every day, and yet uræmic symptoms never appeared. I have found a still lower rate of secretion amongst melancholics who persistently refused nourishment, and yet they were not uræmic. Even a complete intermission of urinary secretion may exist for a certain time without causing uræmic symptoms. A very robust inspector of agriculture suffered from a concretion in the left pelvis of the kidney. After a sharp attack of renal colic, not a drop of urine was passed for one hundred and twenty-two hours, and the bladder was empty, as was shown by the catheter. During this time the patient, no doubt, vomited very frequently; but there was not a question that this symptom was the consequence of peritoneal irritation in connection with the wedging of the concretion in the ureter, and that it was not the result of uræmia. For the vomiting began with the spasm, and there was not the slightest trace of urinary matter nor of ammoniacal salts to be found in the vomit. In the twenty-four hours immediately succeeding the cessation of the spasm, 3,025cc. of urine were passed, of 1,009 sp. gr. This urine contained some blood, and 1·37 per cent., or 41·44 grammes, of urinary ingredients. Some days later a concretion, almost as large as a bean, and made up of urinary ingredients, was passed by the urethra, whilst the patient was urinating. Biermer has observed a similar, long continued, absolute retention of urine without the appearance of uræmic symptoms. There is therefore no reliable minimum standard of urinary secretion, the persistence of which is *invariably* accompanied by an outbreak of uræmic symptoms. Consider, gentlemen, that the quantity of urinary ingredients formed within the body of the same individual must be constantly changing, according to the amount of metamorphosis going on, according to the varied quantity and quality of the food, and according to the varied amount of rest and muscular exercise taken. For every one must admit that the movements of the body, directly or indirectly, owing to the increased requirements of nutrition, exercise a certain influence over the amount of tissue-changes. If, then, metamorphosis in the bodies of *healthy* persons is subject to such variations, we cannot doubt that the production of urinary matter must be subject to still greater fluctuations under morbid conditions of nutrition. We must leave out of consideration the morbidly increased consumption of tissue which takes place owing to *fever*, because these kidney diseases *per se*, run a non-febrile course; the points, then, to be taken into consideration with regard to the inquiry as to the amount of urinary matters that will be produced by a person, are, firstly, what amount of changeable nitrogenous tissue is there remaining in the body? Secondly, what amount of albuminous matter has been taken up by the digestive organs and conveyed into the fluids and tissues of the body? And lastly, what amount of muscular exertion is being undertaken by the individual?

The greater the amount of emaciation which this morbid process has given rise to, the more profoundly the digestive functions and general nutrition are injured, and the less the amount of muscular exercise taken, so much the more slowly does metamorphosis go on, so much the more insignificant is the daily production of urinary ingredients, and so much lower does the demand of the organism upon the depurative functions of the kidneys sink.

But, as before remarked, such a low state of nutrition is much earlier and more constantly found among patients suffering from the inflammatorily enlarged kidney than from the genuinely contracted one. At an early period of the former disease, when the patient is but little weakened, fluid effusions appear to compensate for the diminished secretion of urinary matter at this period of the disease, for the fluid which is discharged into these serous sacs carries with it a portion of the urinary matter circulating in the blood. In the serum of the pericardium in such a case I have found a whole per cent. of urinary ingredients. And I am quite certain that I have never yet seen uræmic symptoms in such patients so long as the dropsy was on the increase, but I have seen them several times when a sudden absorption of the fluid has been brought about by sudorifics or laxatives. At all events it is the exception for the kidneys of a patient who has died of uræmia to be found swollen by chronic inflammation. The great degeneration of general nutrition and the absolute rest of body necessitated by the dropsy which the above-mentioned condition of the kidneys brings with it considerably lessen the necessity for the purifying influence of the kidneys, and, moreover, in many cases the deficiency in the quantity of urinary secretion is to a great extent covered by the abnormally high percentage contents of the same in solid urinary ingredients. Nevertheless, many cases which commenced as inflammatory enlargement of the kidneys, and in which a long-standing dropsy has slowly disappeared but albumenuria continues and in which the whole character of the urine shows a change into secondary contraction, do terminate fatally in chronic uræmia. I have already told you that under these conditions the patient, whose free movements of his limbs is now no longer impeded by dropsy, may again increase in weight and strength; but in such a case as this the functional activity of the kidneys is always on the decrease on account of the advancing process of contraction; although a consecutive hypertrophy of the left ventricle may take place even in these patients, as I have before remarked. But at last the remnants of the kidney substance no longer suffice to free the organism of the effects of metamorphosis, and, under such circumstances, a case of chronic nephritis, beginning with general dropsy, may end fatally in chronic uræmia after the complete disappearance of the dropsy. But it is very seldom that cases beginning in such a manner terminate in uræmia.

But, on the contrary, as soon as the destruction of the tissue of the *genuinely contracted* kidney has advanced to an extreme degree, as soon as the former large daily quantities of urine have diminished and at the same time the percentage of solid urinary ingredients has sunk low, as soon as the fluctuating, but the always returning, dropsical swellings of varied and mostly different portions of the body have made their appearance, owing to the small amount of urine that is being passed, as soon as the former healthy digestive functions have got deranged, and the symptoms of general decay, loss of flesh, and a pale earthy tint of skin, have made their appearance, *most* cases of *genuine contraction* of the kidney terminate in chronic uræmia.

But a sudden attack of acute uræmia is a much more frequent symptom in genuinely contracted kidney than it is in the chronically enlarged kidney; it makes its appearance in the former disease long before the morbid process has rendered the kidney incapable of performing its functions. Sometimes, without any warning, such persons are attacked with epileptiform convulsions of the severest type in the midst of business or pleasure. Their most intimate friends may not have had an idea that they were ill; they may have appeared perfectly strong and healthy up to the outbreak of these frightful symptoms, and may have gone about their business as usual. Very often the convulsive attacks are repeated at very short intervals and with equal severity, and the patient does not come to himself during the stupor intervening between the fits; in which case death usually takes place in the course of a few days or hours. Acute uræmia in such cases, in a certain measure, gives rise to the first, and, because it is fatal, the last, cerebral symptom in this insidious kidney disease. In other cases, the patients recover completely from the first attack of uræmia; they feel quite well and hearty after a time, transact their business again, and, according to my observations, an interval of some months may pass by before a second attack of uræmia, or of apoplexy, puts an end to life. The question is, in what way does this overloading of the fluids and tissues with urinary ingredients come about in these cases of sudden outbreak of uræmic symptoms in the apparently healthy? Anatomical investigations of the kidneys give us but incomplete information; for I have never found the contraction advanced to an extreme degree in the bodies of such persons as have died from a primary attack of acute uræmia; and clinical observation teaches us that in isolated, not immediately fatally ending cases, the kidneys appear capable of carrying on such a secretion of urinary ingredients as is necessary for the preservation of good health for a long time after the first attack. Unfortunately, my anatomical investigations are incomplete in one respect; I have omitted to remark, in these cases of sudden death from uræmia, whether the straight urinary tubules were not stopped by fibrin casts in some unusual manner; these casts are seldom found in the straight tubules in genuine kidney contraction. Clinical observation of one case in which the patient survived the first attack of uræmia for a twelvemonth does not favour this view, for the portions of the urine secreted immediately after the attack, like the usual urine from contracted kidneys, contained but a sparing quantity of small fibrin cylinders. What amount of solid urinary ingredients are passed before the outbreak of acute uræmia one but rarely knows, for before that catastrophe there is usually neither cause nor opportunity for the employment of exact urinary examinations; and these would be worth nothing with regard to the settlement of this question unless the analyses had been carried on for several weeks. For this urinary matter must take some weeks to accumulate within the body, because, as the case I mentioned before shows, a complete interruption of the urinary secretion may be endured by a healthy man for a week without giving rise to any uræmic symptoms. One must bear in mind that in these cases one of two things has occurred, either the secretory functions of the kidney have become obstructed in some way or other, or peculiar circumstances have made a sudden demand upon the functions of the diseased kidney. In exceptional cases we can show that such interruptions of the secretory activity of the kidneys have occurred, or we can point to circumstances which have brought about an increased production of urinary ingredients, and in this manner we can trace an indirect cause for the uræmia. The case I have already brought before you is an example of this; here a girl afflicted with contracted kidneys was attacked by fresh endocarditis, which immediately brought on an incompetency of the mitral valve. In consequence of this the daily quantity of urine sank during the month preceding death, whilst she was suffering from acute uræmia, to an average of 763cc., which contained, on an average of twenty-five analyses, only 12·7 grammes of urinary ingredients, although the patient was very feverish, the evening temperature often reaching 40° C. Also, long-continued depressing emotions may, by their influence upon the heart's action, indirectly impede the functions of the kidney. A gentleman who in his official capacity had to struggle against perpetual annoyance for a long time, was attacked with uræmic convulsions, from which he nevertheless recovered and performed his official duties for nearly a twelvemonth, when he succumbed to an attack of apoplexy. After death I found the kidneys moderately contracted.

In many cases of sudden death from uræmia, one may search in vain for the cause of its occurrence. But in the consideration of the fact that acute uræmia occurs much oftener as the result of contraction of the kidneys than of inflammatory enlargement, although in the former case more urinary ingredients are secreted than in the latter, we must always bear in mind that contraction of the kidney reduces the general health but little, and that it very often occurs in a robust person whilst rapid metamorphosis is taking place and the patient exerting himself exactly as usual, and that consequently it occurs in a person under circumstances which demand the full activity of the organ entrusted with the secretion of the urinary products, whilst these very organs are in such a condition that under any circumstances they are only able to supply a secretion relatively poor in specific excretory matter.

Having seen what a difference there is between the two foregoing kidney diseases in their effect upon the general nutrition, we will now endeavour to find some true explanation for it. In all cases where the decay of general nutrition has to be explained, we take it for granted that there must have been some disturbance of equilibrium between the supply of and demand for the materials of nourishment; we have then to inquire whether there have been any morbid disturbances of digestion or assimilation to retard the conveyance of these materials into the system; also, whether there has been an unnatural withdrawal of assimilated material from the body; and, whether an abnormally high rate of consumption of materials has been going on owing to anomalies of metamorphosis, to which we can attribute the loss of equilibrium. An unnaturally high rate of consumption in these two forms of kidney disease is impossible, for they run their course without fever. Disturbances of digestion are absent at the commencement of both forms of the disease in the greater number of cases; they mostly appear later on, though, in some cases, they may be absent throughout. And yet, what a constant difference there is, in the earlier period of the disease, between the patients belonging to one group and those belonging to another with regard to nutrition. The one afflicted with inflammatory enlargement of the kidney soon becomes pale and anæmic, his muscles waste away, his strength fails, and often the wasting of his whole body is only hidden by the dropsical distension of the entire subcutaneous tissues. The patient attacked with genuine contraction of the kidney, on the contrary, may preserve a healthy appearance, strong muscles, pretty fair strength, and full contour for years. An exact investigation of the urine affords us a perfectly satisfactory explanation of this difference in the nutrition of these two classes of patients. Both forms of disease are accompanied by *albumenuria;* thus, in both, a portion of the already assimilated material, which by means of metamorphosis gradually becomes the substance of the body, is incessantly removed. But the loss of albumen through kidneys undergoing *chronic enlargement* is incomparably greater than that which takes place through those undergoing *genuine contraction*, and not until the swelled kidney has become wasted by secondary contraction does this loss of albumen become as slight as it is during genuine contraction. The daily quantity of urine in the earlier period of kidney enlargement is very slight certainly, but the percentage of albumen may mount up to three per cent. and higher, so that the total loss of a daily secretion may rise to 20 grammes, and over that. One of my patients, on an average of fifteen analyses made in the course of a month, lost every day 14·5 grammes of albumen with the urine; and a little slim girl, on an average of eighteen analyses made in the course of two months, lost 15 grammes daily. "Let one take what view one may as to the uses of albumen," says Voit, "it at all events influences the amount of active cell-growth and the supplies of oxygen, on which again decomposition, and therefore movement, depend."

If we now compare the amount of albumen which is contained in the urine from contracted kidneys with that which is contained in the urine from the inflammatorily enlarged kidney, we find very often that in the former case the urine does not contain so much per thousand, as in the latter it does, per cent.; very often indeed the former does not contain anything like one per thousand, and at times albumen may be altogether absent. In spite of the copious urine, the total loss of albumen through the kidneys in these cases is always very slight compared with that through the inflammatorily enlarged kidney; it is limited to one or two grammes, and it may amount only to a fractional portion of a gramme for a single day. One of my patients, on an average of twenty-one analyses made in the course of three months, lost 1·3 grammes of albumen every day; another, on an average of eleven analyses, extending over a longer period, lost 1·4 grammes; a third, on an average of twenty analyses made in the course of a month, lost 1·2 grammes. Experience in some of these cases has shown me that so slight a loss of albumen from the human organism may be endured without essential injury to the general nutrition; for example, the first of the foregoing patients is alive now, two years after the performance of those analyses, and walks along with a firm step, in spite of his continued albumenuria.

If we now endeavour to advance a step further with our investigation, and ask what are the causes of the albumenuria in these two diseases of the kidney, I am sorry to say I am by no means in a position to give you a satisfactory reply. That the secretion of albumen with the urine must, under all circumstances, be considered a pathological process, no one any longer doubts. With regard to this it appears to me indisputable that the migration of this material from the serum of the blood into the urine may take place in various ways, and that, in the two before-mentioned kidney diseases, it certainly does take place in two different ways. Physical experiments on filtration show that animal membranes which, under a certain moderate pressure of fluid, are impermeable to colloid substances—*e.g.* albumen—will, under *increased* pressure, let small quantities of such substances through. In *contracted* kidneys we find the filtering apparatus—the Malpighian corpuscles—in a thoroughly normal condition wherever it is permeable by the blood. But, as was shown before, they have to resist a blood-pressure *increased* beyond the normal, and, in consequence, a small quantity of albumen passes through their pores in addition to water, urinary ingredients, and salts. The urine from contracted kidneys thus resembles a fluid which has transuded under abnormally high blood-pressure; it is similar, for example, to the ascitic fluid which results from obstruction of the portal vein, only that it has urinary ingredients mixed with it. The albumen may entirely disappear from the urine of contracted kidneys during the diminution of arterial pressure which occasionally takes place; and it always decreases very considerably as the quantity of urine diminishes; for with the advance of the pathological process in the kidneys a general decay of nutrition and a continued loss of power in the heart take place.

The enormous amount of albumen which *chronically* *enlarged* kidneys often secrete is much more difficult to account for than the albumen from contracted kidneys. Of the relations of the blood-pressure in such kidneys we know nothing certain; we can only conjecture that, partly on account of the slowness of the blood-stream through the inflammatorily changed vessels, and partly on account of the obstructions which the blood-current in the capillaries of the cortex of the kidney meets with, owing to the swelling of the interstitial tissue and the epithelium, that this pressure must increase above the normal in the Malpighian corpuscles. The pressure in the entire aortic system is, on the contrary, lessened, at all events in most cases; thus by no means can it be the cause of the albumen in this kidney disease. We do not know much more of the nature of the filters through which this richly albuminous secretion passes than we do of the relations of the blood-pressure with them. Virchow mentions that he has often found the loops of glomeruli broader and duller than natural, and that on the addition of acetic acid they displayed a thickened wall and numerous nuclei which exceeded the normal in number three or fourfold perhaps. Also, Colberg showed me a preparation in which the Malpighian corpuscles appeared much larger than natural. Thus there were conditions such as we are acquainted with generally in vessels changed by inflammatory irritation. But the microscopical condition of the kidneys, the enlargement and multiplication of the cellular elements, and the swollen condition of the epithelium in the uriniferous tubes, show that inflammation has given rise to this enlargement of the kidney, though these changes might be considered to arise entirely from an inflammatory condition of the nutritive vessels. But the above-mentioned condition of the Malpighian corpuscles, noticed by Virchow and Colberg, would seem to indicate that the functional vessels also take part in these changes; at all events, the nature of the urine secreted by such kidneys corresponds to this view. According to Cöhnheim's well-known discovery, not only blood-plasma, but also white blood-corpuscles, pass through the walls of inflammatorily changed vessels. We find these constituents in the urine secreted during diffuse nephritis. The richness of such urine in albumen (it may reach 5 per cent.) may exceed that of the fluid drawn from the skin of a man by a blistering plaister, and we must acknowledge that this fluid is an undoubted inflammatory product. The copious cylindrical fibrinous casts, and the by no means sparing quantity of white blood-corpuscles in the sediment of this urine, leave, it appears to me, no doubt that an inflammatory transudation gets mixed with the secretion of these kidneys, and that the albumenuria resulting from diffuse inflammatory enlargement of the kidneys must arise from an inflammatory change in the filter through which the urine passes out of the blood.

That the general nutrition in the course of these two kidney diseases may be injured both by the loss of albumen and by the interruption of digestion, I have repeatedly and specially pointed out. But in the latter respect the patients suffering from the one disease differ from those suffering from the other. In patients suffering from *chronic enlargement* of the kidney, loss of appetite, malaise, sickness, more rarely diarrhœa, sometimes make their appearance during the first attack of dropsy, and no doubt in such cases considerably assist the sudden decay of general nutrition. But with the disappearance of the dropsy in these patients the foregoing symptoms may disappear and the organs of digestion may resume their normal functions whilst the general nutrition is immediately restored to a fair condition, until, with the advance of supplementary contraction, a second, and now continued, disturbance of the functions of these organs gradually makes its appearance. In *genuine contraction* of the kidney, digestive derangements, loss of appetite, obstinate vomiting and diarrhœa, never make their appearance before the disease has reached an advanced stage. The question now is, whether digestive derangements, under such varied conditions, have a similar cause; I think not. With regard to the obstinate vomiting which occurs in patients afflicted with kidney *contraction*, whether in the genuine or in the supplementary form, I have endeavoured to justify the theory which has been advanced for a long time by various writers, that it is of a uræmic nature, and I have appealed to the fact that we can demonstrate urinary matter, or the product of its decomposition—carbonate of ammonia—in the vomit of such patients; and, further, to the circumstance that obstinate vomiting very commonly precedes uræmia arising under other circumstances. The same explanation does not hold good for the vomiting of dropsical patients suffering from *chronic* kidney *enlargement*. At least I have not succeeded in showing urinary matter or ammoniacal combinations in the watery and sour vomited matter of such patients, and no uræmic symptoms followed the retching in my cases. I think a much more correct notion is to consider that these interruptions of the digestive functions of patients who are highly dropsical are the consequences of an œdema of the mucous membrane of the intestinal canal. Unfortunately I have not had an opportunity of testing the correctness of this view by an examination of the body after death. But I can very well conceive that such an œdema of the mucous membrane may occur in both the intestinal tract and the air-passages. And it is evident that quantities of water might transude, instead of this œdema of the mucous membrane of the hollow viscera taking place.

The presence or absence of *hypertrophy* of the left ventricle of the heart evidently stands in intimate relation with the general condition of nutrition. Traube has most convincingly shown that this change in the heart originates in an increase of blood-pressure in the aortic system, the necessary consequence of the obliteration of so many vessels for the exit of the arterial blood from the kidneys. Hypertrophy of the left heart is an almost constant companion of the genuinely contracted kidney; it is, on the contrary, as a rule, wanting in patients suffering from chronic enlargement of the kidney. And yet we are by no means certain that the movement of the blood through the kidneys is more disturbed by the process of contraction than it is by the inflammation which gives rise to chronic enlargement of the organ. The strikingly pale and anæmic appearance of the swelled kidneys points much more to a high degree of anæmia during life, a consequence probably of the pressure exercised upon the blood-vessels by the swelling of the parenchyma. In the one case, however, in *genuine contraction*, the hindrance to the circulation which gives rise to the increased labour of the heart developes itself quite gradually in a body existing under favourable circumstances of nutrition, and furnished with undisturbed blood supplies; in the other case, on the contrary—in *chronic enlargement* of the kidney—this hindrance developes itself in a body the blood of which suddenly becomes poor owing to a proportionally large loss of albumen, and which blood is thus unable to supply the means for extra nourishment to the heart, and in a body in which the tension in the aortic system never reaches a higher point, owing to the amount of water which has transuded through the vessels. But the accompanying dropsy disappears if in the course of time the inflammatory swelling passes into supplementary contraction; and if the loss of albumen through the kidneys becomes very slight, and an improvement takes place in digestion and general nutrition, hypertrophy of the left ventricle may become associated with this secondary contraction of the kidney. Indeed, this hypertrophy sometimes takes place during the existence of the inflammatory enlargement; in such cases the general nutrition may have been exceptionally little injured.

Hypertrophy of the left ventricle derives its chief practical interest from its connection with cerebral apoplexy; a mode of death very frequently observed during contraction of the kidney, which, on the other hand, has not occurred in any of my cases of inflammatory enlargement. As Traube has already shown, the connection between hypertrophy of the left ventricle and cerebral hæmorrhage evidently and undoubtedly arises in consequence of the increase of the arterial blood-pressure in connection with the change in the heart; and this increased pressure, under certain circumstances, brings about a rupture of the cerebral vessels. It is, on the contrary, very questionable whether other hæmorrhages besides those in the brain, such, for example, as those which by no means rarely occur on the various mucous surfaces during the foregoing kidney disease, are to be traced back to the same cause, viz. hypertrophy of the heart. After greater experience, I consider such a causative connection very improbable. With regard to the relation of this change in the heart to the before-mentioned retinal mischief in chronic kidney disease, I cannot say anything from my own personal observation; hitherto I have not had the opportunity of a sufficiently accurate study of this question. But it is Traube again who urges the dependence of this disease of the retina upon the hypertrophy of the left ventricle, and the therewith increased tension of the aortic system; and I am indebted to my colleague, Völchers, for the communication, that amongst all the cases of Bright's retinitis (perhaps twenty in number) observed by him, only one occurred which was complicated with dropsy. The patient, a pregnant female, quite recovered from her kidney mischief. In the majority of cases the kidney disease, which had not hitherto been diagnosed by the family doctors in attendance, was first discovered through Volchers' ophthalmoscopic investigation. However, Cöhnheim repeatedly saw Bright's retinitis without simultaneous hypertrophy of the heart.

Gentlemen, by means of morbid types, I have attempted to describe to you shortly the clinical course of two important pathological processes in the kidneys, which I consider essentially different from one another, but which many writers describe as merely different stages of the same inflammatory process. I have then endeavoured to show you that the particular morbid symptoms can in each case be traced back to interruptions of the physiological functions, which interruptions are connected with anatomical changes in the kidneys. But however characteristically these morbid types may shape themselves in most cases, and however safely we may in these cases judge of the anatomical condition of the kidney from the symptoms of the disease, yet I must not conceal from you that transitionary forms between the two kidney diseases distinguished by me, or combinations between the one and the other, do undoubtedly occur. Post-mortem examinations show that in isolated cases one kidney may be contracted, the other inflammatorily enlarged. That such cases place insuperable difficulties in the way of our diagnosis is very evident. We do not hide from ourselves that there are still great gaps in our knowledge and ability in this department of pathology; but let me conclude with the wish that further researches may very soon succeed in filling these up, and that an increasing knowledge of pathological facts may open the way to more beneficial treatment.

CATARRH

OF THE

FEMALE GENITAL ORGANS.

BY

PROF. H. HILDEBRANDT,

OF KÖNIGSBERG.

Gentlemen,—I daresay you will have observed, in the course of the clinical investigation which we have made together, that there is hardly any complaint so widely spread amongst women as that which, in contradistinction to the physiological sanguineous discharge, they call the "Whites." I can assure you that half the women who frequent the consulting-room of an obstetric physician come solely on account of this troublesome symptom.

We still frequently meet with the opinion, which I am sorry to say, in spite of the numerous cases of women's diseases we have examined in this place, I have discovered to-day amongst yourselves, that that usually whitish, often very copious, weakening discharge, is merely a vaginal secretion, a symptom of vaginal catarrh. In opposition to this view I must, first of all, and *à priori*, mention that simple catarrh of the vagina seldom occurs, that the real seat of the mischief is to be sought for deeper—in the uterus and its appendages, but very frequently also at the mouth of the genital canal, in the vestibulum.

The way in which you can prove that this is the case is twofold; first, by a microscopical and chemical examination of the secretion; and then by a local inspection of the genital passages by means of the speculum.

The secretion which leucorrhœa supplies varies very much in regard to its colour, consistency, and odour; at one time white, creamy, and thickish; at another, more purulent, or intermixed with traces of blood, often quite glassy, viscid, glairy, and glutinous, but sometimes also greyish-brown, in which case it has usually a disagreeable odour, similar to that of the greasy lochial discharge of the lying-in woman who is suffering from the retention within the cavity of the uterus of decomposed portions of the fœtal membranes and clots. If you place a portion of such secretion under the microscope, as I have done here for your inspection, you will find numbers of variously-shaped epithelial cells in the more or less cloudy serum. Pavement epithelium is most abundantly represented, cylindrical epithelium next, and then the ciliated. Amongst these are to be found mucous corpuscles, and, in many cases, numerous pus-corpuscles and red blood-discs. Vibriones often cross the field of sight. And, in addition, peculiar forms of infusoria and fungoid growths are to be met with. Under the next microscope which I have placed here for you is a little secretion from a pregnant female suffering from vaginitis granulosa. You will see that there is abundance of pavement epithelium from the vagina, some cylindrical epithelium from the cervix, which is ulcerated on both lips, two small mucous corpuscles on the right hand of the field, and somewhat distant from these, towards the left, there is a small, roundish, somewhat long body, which much resembles a mucous corpuscle, but which is provided at one end with two whip-shaped appendages; it is the so-called trichomonas vulvæ. You will recognise these infusoria by their lengthened shape and the appendages at the cephalic end; there are not usually more than two of these appendages, sometimes there is but one. You recognise them also by their movements, which are well seen in this preparation, for we have prevented the fluid drying up, and the infusoria have been left in the element favourable to the preservation of their life—the acid fluid of the vaginal secretion. Fungoid growths are found mixed with the secretion not less rarely than infusoria, more especially the leptothrix vaginalis, which often occurs in great quantities and in unusually large bundles; oidium albicans has also been found in both the forms in which it has been found in other organs, but more especially in the mucous membrane of the mouth. You will find a specimen of oidium albicans under the next microscope. These specimens come from an old woman, whom I have brought here for your inspection. She has suffered for a twelvemonth from diabetes mellitus and acute pruritus vulvæ, a combination I have seen once before in an elderly person. In this case the saccharine urine is evidently the cause of the fungoid growth in the genitals, and the latter gives rise to the pruritus. Locally, with the unaided eye, you will see nothing more than a somewhat increased secretion, an itching erythema of the labia, and a superficial redness of the vagina; the anterior wall of the latter is somewhat sunken, and consequently the urine trickles over it the more readily.

With regard to the leptothrix vaginalis, you will be interested to learn, that according to the researches of Haussmann, this fungus cannot be got by transporting the penicillium glaucum to the vaginal mucous membrane, as might have been supposed from Hallier's work. With regard to the oidium albicans, Haussmann has made the interesting discovery that when spores of it are introduced into the mouth of the new-born infant they give rise to thrush (Soor); an observation which explains the striking fact that children, even in the first day after birth, are sometimes attacked with this disease. Evidently in these cases earlier existing spores, which have got mixed with the vaginal secretion, have reached the mouth of the child, whilst the head was detained in the vagina a longer time than usual during the expulsive stage.

If you now bear in mind the various elementary forms which the microscope shows you in the morbid secretion of leucorrhœa, you must be convinced that I was quite right when I asserted that we had to do with affections of anatomically different mucous membranes, and not with those of the vagina alone. In addition, the vagina possesses hardly any glands, only a few crypts in the mucous membrane of the posterior wall, and yet the morbid secretion is often tenacious, sometimes like the white of an egg, and the microscope shows large mucous globules. Furthermore, we see numerous representatives of the ciliated and cylindrical epithelium, although the vagina is supplied with nothing but pavement epithelium.

The albuminous sticky substance with the numerous mucous globules results from disease of the *cervix*. The appearance of copious quantities of ciliated epithelium under the microscope indicates disease of the mucous membrane of the *corpus* uteri, and perhaps that of the *tubes*, whilst a too copious appearance of cylindrical epithelium points to a lengthened catarrh of the *cervical* mucous membrane. Cloudy serum, pavement epithelium, infusoria, and fungoid growth, are the microscopical elements of *vaginal* catarrh.

A *chemical* examination of the secretion does not give us so distinct an indication as to the seat of the mischief as we get by the microscope. But you may generally conclude that a strongly *alkaline* secretion belongs to the *cervix*. The secretion from the *vagina* alone is, on the contrary, always *acid*. When by suction with a syringe we succeed in obtaining perfectly pure secretion from the mucous membrane of the *body* of the uterus, we find that it is usually *neutral;* it seldom has an *acid* reaction.

We shall, nevertheless, always take the shortest and safest means for the determination of the seat, the fountain-head, of the catarrh, if we make an exact ocular inspection, in which we are obliged to employ the mirror.

I shall now make a digression, in order to describe to you, or, at least, to thoroughly criticise, the various forms of specula which we make use of in these investigations, and with which you are already acquainted from my theoretical lectures. Instead of extolling so many new forms, and, above all, in order to guard you from the errors into which some of my colleagues have fallen, who gradually go too far in their reverence for Sims' or Sims-like instruments, I cannot omit to impress upon you that, whether used in the patient's own room, or in the obstetric physician's consulting room, Carl Mayer's porcelain tubular speculum and Fergusson's similarly shaped glass speculum, with an outer casing of gum and an interior lining of tinfoil, are still always found to be the most serviceable, the most convenient, the handiest, the least disagreeable to the patient—because they do not outrage her modesty—and the least painful in their application. These tubular specula give us abundance of light, and can be introduced without difficulty; they give rise to little or no pain when we have them cut off obliquely at the end and they are inserted in a somewhat rotatory manner with the lip of the instrument directed towards the posterior wall of the vagina. If we do not choose too long an instrument, a fault which is often committed, and if we do not take one of too small a calibre, we can generally without any difficulty examine the whole vaginal portion of the uterus in every direction, and during its gradual withdrawal we can bring into view bit by bit every portion of the vagina. But it certainly cannot be denied that these instruments do not suffice in many displacements of the uterus and in many diseases of the vaginal cavity, for, in the first case, the vaginal portion of the uterus evades the aperture of the speculum, and must be first dragged down forcibly with the sound into the lumen of the tube, which operation may be attended with disadvantage as regards the diagnosis, and with acute irritation of the cervix; whilst, in the latter case, the lumen of the speculum gives us too small a field of observation. Sims' duck-bill speculum, with which you are already quite familiar clinically, is recommended in these diseases, and because we do not know beforehand what difficulties of the kind we may have to encounter, it is recommended to be used in all examinations. These specula, which moreover are very similar to the long-known ones of Simon, are truly one of the best inventions in clinical gynœcology of modern times, and their employment has become absolutely indispensable at the bedside, especially in most operative procedures for diseases of the uterus, the bladder, and the vagina; but they require the help of two assistants, and even with the utmost management and the addition of the very recently recommended, though not very useful elevators of the anterior wall, they require the help of one assistant at least. And for this very reason they are as good as useless in the consulting-room of the physician, at least amongst German women, for when a vaginal examination is required, they will not, except with extreme reluctance, permit the presence of a second physician, or even that of midwife.

But an instrument has been brought out after the pattern of Sims' speculum, which has its advantages without its faults, when used in private practice; this is the two-bladed speculum of Cusco, which can be managed without the help of an assistant, and which, during an examination, affords equally as large a field of view around the vaginal portion of the uterus as the awkward, indecent, and painful duckbill of Sims. You will do well, therefore, to secure one of Cusco's specula as well as several of Mayer's of various sizes, the ends of which have been obliquely ground off.

We will now at once proceed to the changes which we find on an examination with the speculum; but let me tell you beforehand that at first we intend to occupy ourselves with the phenomena of *chronic catarrh* only, and that a description of the very much less frequent acute catarrh will be given afterwards.

You will find that in the larger number of cases the leucorrhœa originates in the vaginal portion of the uterus. But this structure, as seen through the speculum, varies very much in its appearance, according as we have to do with it in the woman who has had children, in the sterile female, or the virgin. It is therefore absolutely necessary that I should draw your attention to the essential points of difference. In virgins and childless women we find the vaginal portion unusually long, thick and tumid above, and tapering to a point below. The colour is mostly deep dark-red, sometimes bluish red; the orifice exhibits annular epithelial excoriations, sometimes annular ulcers; a drop of glassy, viscid, perhaps somewhat purulent secretion may hang from the os. In women who have borne children the vaginal portion is cylindrical throughout; at the transversely fissured orifice it may be broader than it is above; it is also deep dark-red, and the broad lips of the os, which are usually somewhat everted, are always eroded when the disease has existed for any lengthened period. If we make a digital examination of the cervical canal, which is generally pretty open, we find its mucous membrane, like that of the lips, puffy and spongy like velvet, and here and there scattered over the surface of the mucous membrane more or less knotty or rounded granular projections are often found. These protuberances are seldom absent in a case which has lasted for some time. Moreover they are better felt than seen, for all attempts to construct specula for the cavity of the uterus are frustrated by the inconvenient hæmorrhage of the mucous membrane which they give rise to. They result partly from hypertrophy of the cervical glands and partly from a morbid enlargement of the cervical papillæ. Thus, in both groups of cases—in females who have not borne children, and in those who have—the characteristics of the disease are: swelling of the mucous membrane, with secondary sympathy and enlargement of the whole cervix; an increase, and frequently a thickening of the glassy secretion; an erosion of the epithelium; ulcers; and a hypertrophy of the glands and papillæ which have been laid bare in consequence of the removal of the epithelium.

This apparently unimportant disease, which occurs in so insignificant a region, and in an organ otherwise so very little sensitive, gives rise to both local and general bad consequences almost without exception, and may even have a fatal termination; nevertheless, the symptoms vary according as we have to do with it in virgins, nullipara, or multipara.

When cervical catarrh exists for a long time in virgins, or in women who have had no children, the menstrual discharge at first increases, owing to the greater amount of blood which flows into the organ; it becomes irregular and reappears too often, without giving rise to any pain, at least at the commencement. If there are ulcerations, we generally get intercurrent hæmorrhage, in consequence of some unusually violent exercise, such as dancing, springing, running probably because the ulcerated spot on the cervix rubs against the posterior wall of the vagina, across which numerous deep folds stretch. Later on, when the ulcerations have existed for some time, narrowing of the external os occurs, for the ulcerations partially heal over and form scars, the contractions of which diminish the aperture of the cervix too much. A necessary consequence of this is, that the viscid, slowly moving secretion of the cervical glands stagnates, and, owing to the impediment, distends first of all the cervical canal, and then the cavity of the corpus uteri. The whole uterus usually enlarges both in length and breadth; it looks like a thin loose-walled balloon, out of which the secretion is from time to time evacuated with pains like those of labour; this secretion, which is now mixed with that of the secondarily diseased mucous membrane of the body, is a yellowish, often purulent, more or less dark-coloured fluid. But as soon as such a balloon-like distension of the uterus has taken place, further severe consequences almost always follow. This thin-walled loose uterus is unable to resist the pressure of the intestines pressing upon it from above; it gives way and doubles back into Douglas' pouch; in this way retroflexion of the uterus is a very common secondary result of chronic catarrh in virgins.

These females are almost invariably childless after marriage. Even when they are married before the mischief has advanced beyond the first stage, and when there is only swelling and hypersecretion of the mucous membrane, the semen cannot reach the uterus. Under healthy conditions the folds of mucous membrane which form the palma plicata project pretty considerably on the anterior and posterior walls of the cervix, and when they are morbidly swelled, they dovetail so closely together that the canal may be completely closed. When firstly owing to inspissation of the fluid and secondly owing to contraction of the means of exit a stagnation of the secretion occurs and in addition we get a flexion of the distended loose-walled uterus, we have simultaneously three extremely unfavourable conditions for the passage of the semen into the cavity of the corpus uteri. But where cervical catarrh has existed for a long time in young girls, such serious symptoms gradually make their appearance that, taking their great bodily weakness into consideration, we cannot wish that they should be married or be blessed with children. The first secondary symptoms, which usually make their appearance soon after the beginning of the discharge, are nervous affections of the stomach, cardialgia, and loss of appetite alternating with frequent and most tormenting ravenousness which cannot be appeased owing to the sensitiveness of the stomach. Alternating with these we often get retching for no apparent reason. There are thus in reality those affections of the stomach which we are accustomed to see during the first months of pregnancy; which is not surprising, for during pregnancy the retching very frequently results from a diseased condition of the cervix, more especially from ulcerations of the same. Sooner or later irritability of the bladder associates itself with these disturbances of the stomach, and if the patient has been accustomed to sleep soundly, she will sooner or later be disturbed by the frequent desire to urinate; which is evidently the consequence of the extension of the local inflammation from the cervix to the posterior wall of the bladder. Usually also, painful affections of the uterine nerve smake their appearance, though not so much so whilst the patient is resting: it is when the patient is going about and the vaginal portion of the uterus is pressed down owing to the weight of the superincumbent intestines, and when—irritated by movements on uneven ground—the raw os uteri rubs against the posterior wall of the vagina, that we get neuralgias which have their seat first on one side of the hypogastric region and then on the other, often extending down the thigh. These pains gradually make movement in the open air, as well as domestic occupations, very painful to the patient. They shirk exercise: they make the sofa their favourite resting-place.

Thus maladies develope from local affections of this little cervix to whose importance as regards the life of the female one is apt to attach very little importance, because they are as good as not concerned in and are inactive during the most important physiological functions of the female organisation—during menstruation and child-bearing—and yet these very disorders are quite capable of undermining the constitution. The appetite and the possibility of taking sufficient nourishment disappear; loss of sleep occurs: and finally free movement in the fresh air is hindered; facts of sufficient importance to ruin the health of the strongest person. There is no doubt that a very large number of our obstinate cases of chlorosis and a very great portion of our cases of hysteria owe their sufferings, their weakness, their poorness of blood and emaciation entirely to the neglect of a cervical catarrh; and I am quite certain that this is the more frequent course which the general disease takes with those young women afflicted with chlorosis and leucorrhœa, and not the reverse of this, viz. that the chlorosis exists first of all, and, with this as its foundation, the genital catarrh developes itself, as has hitherto been the general opinion.

Cervical catarrh in women who have borne children takes a very different course to what it does in virgins. Whilst in the latter the dangers of catarrh consist in the tendency to secondary sympathy in the body of the uterus, in the tendency to a destruction of the power of conception, and in the tendency to nervous affections in various regions remote from the uterus, the former—women whose cervix has been stretched by frequent labours and torn at the lateral angles so that an inverted, cone-shaped canal is formed, out of which the secretion easily escapes—run but little danger of a disturbance of their powers of conception. Neither does the general health suffer in these women as it does in virgins, unless, as is not very rarely the case, weakness and emaciation ensue from the great loss of albuminous fluid. But amongst married women wide-spread annular degenerations of the mucous membrane of the cervical canal very frequently occur, most frequently on the lips of the os, but they may be high up in the cervical canal, as far even as the internal os. Where a cervical catarrh has existed for years in a married woman who has had children, there is always ulceration of one or other of the lips. It is usually the anterior, because this lies lower, and over it therefore the alkaline secretion of the cervix continually flows. If these ulcerations continue for a long time they gradually increase in size, so that the whole anterior lip of the vaginal portion is changed into a suppurating surface, the discharge from which mixes with the glassy secretion of the cervical catarrh. Sooner or later little *protuberances* arise on this ulcerated surface which may differ very much in their importance.

In some women the *glands* embedded in the mucous membrane get hypertrophied owing to the continual abnormal irritation, and form larger or smaller excrescences with cyst-like cavities; the coats of the glands get distended and finally lengthen out into those long-stalked, deep, dark-red soft tumours which we call mucous polypi; they may occur in great numbers, both in the inferior portion of the cervix, and high up in its interior, as high as the internal os. A few of these reach the size of a pea, a hazel nut, or a cherry, but they seldom exceed this.

In other women again, the *interstitial tissue* between the glands increases to an unusual extent, projects above the level of the remaining mucous membrane in little circumscribed patches, is provided in its interior with small cavities which contain a serous fluid, and thus likewise forms little tumours, some of which have a broad base, whilst others have a stalk: these are also called mucous polypi. All we know about these two forms of excrescences is that they are especially common on strong, spongy, broad, thick, very hyperæmic, soft cervixes; their bad effects are limited to a constant, very profuse discharge of serous fluid from the cervix, and to extremely copious hæmorrhages; the latter of which are due partly to an increase of menstruation, but sometimes they come on irregularly, even after any strong mechanical "insult," in which the numerous, easily torn capillaries of these little tumours are burst and torn owing to the congestion and over-distension, or through mechanical tearing and squeezing. But much more injurious in its effects is a third form of excrescence which arises on the ulcerated mucous membrane of the cervix during catarrh, the so-called *papillary growth.* These papilloma are particularly common in women, the vaginal portion of whose uterus is hard, resistent, and not very large. If the orifice is not widely cleft, all we notice is that on its borders, and therefore on the margins of the lips, there are larger or smaller, somewhat longish cone-shaped projections, which, taken together with the lips, resemble a cock's comb. If the orifice is wide open, cleft, and the lips everted, the first thing we observe at the commencement of the growths are little projections the size of a millet-seed, which easily bleed when touched with wadding. These little projections extend upwards into the cervix, and frequently they may be followed as far even as the internal os. Later on they widen more and more at their base; even in the canal they get cone-shaped, ramify, and swell out knobbily at their extremities; changes which are brought about much more slowly in the canal than on the lips, on account of the narrowness of the former and the absence of direct mechanical irritation.

These papilloma on the mucous membrane of the cervix and the lips of the os are of the utmost importance. For experience teaches us that these excrescences when left to themselves very frequently become cancerous. In so far as my experience has taught me hitherto, I should say that very much more than one-half of all the cases of primary cancerous degeneration of the vaginal portion of the uterus originate in the mucous membrane of the cervix, and date their commencement from simple catarrh accompanied by papillomatous ulcers undergoing malignant degeneration. As illustrations of these forms of disease I will relate two examples.

Five years ago last May, a perfectly healthy, strong country girl, in her twenty-first year, whose parents had always been healthy, was delivered in our obstetric department after sixteen hours' labour, and after we had used considerable force with the forceps. She had severe endometritis for a week, but was discharged on the sixteenth day after the labour with the body of the uterus well reduced to its normal size. She complained a little of a considerable, tenacious, purulent discharge that still came away. It came from the thick, flabby cervix. The patient was told she must take care of herself, and, above all, avoid heavy work. In the beginning of September of the same year she came back to the hospital because the discharge still remained, and because there were often not inconsiderable traces of blood mixed with it. She had quickly got well when she returned to the country in the spring, and had even now a strong, fresh, healthy appearance. But as soon as she returned home she had very heavy work to do, and the more laborious it was the more abundant was the discharge; but this she did not think very much of until she was alarmed by the streaks of blood. An examination revealed a cervix thickened in every direction, but universally soft and spongy; both lips were everted, the posterior soft and velvet-like, and bleeding to the touch. With the speculum one could see on the posterior lip a roundish ulcer of about 3 ctm. diameter, the surface of which had that finely granular, easily bleeding character which is peculiar to the commencement of the papillomatous ulcer. This spot was cauterised once, and the patient was told that she must return to the hospital at the end of October. Unfortunately she exceeded this time. She did not return until the March of the following year, and certainly with a very suspicious aggravation of the mischief. The ulcer was still limited to the posterior lip, but its surface was considerably swelled, owing to the exuberance of the granulations. The lip was markedly hypertrophied by infiltration of the submucous tissue; but the most alarming thing about it was that at the spot in the vagina where this diseased lip had rested posteriorly, and on this spot alone, with every other portion of the vagina perfectly healthy, thus showing plainly that it was the effect of the superincumbent diseased mucous surface, the vaginal mucous membrane was not only deprived of its epithelium, but in its place a perfectly similar papillomatous ulcer had made its appearance. We cauterised deeply with chromic acid, and later on we burnt the spots repeatedly with the red-hot iron, and, as we thought, quite down to the healthy parts. Nevertheless no good granulations appeared on the surface of the ulcer. On the contrary, and this is so truly characteristic of the malignant ulcer, the more we took away the faster grew the diseased tissue beneath. In short we had a clinical demonstration of one of the most instructive and characteristic cases of cauliflower growths. She died six months later of cachexia. An examination of the genitals showed that it was a case of rapidly growing soft cancer, which had involved the entire vaginal portion of the uterus and the upper and posterior portion of the vagina.

A fit companion to this case is that of a youngish lady, with a good family history, the mother of a healthy boy, who nevertheless had aborted frequently, as it appeared, in consequence of a chronic endometritis, the result of a concealed gonorrhœa from which the husband suffered. When the patient first consulted me—it may be ten years ago—I found the following condition:—The portio vaginalis was very hard and thick, both lips were slightly ulcerated, there was a copious acrid leucorrhœa, chlorosis, a generally nervous state, and an unhealthy appearance. After the prescription which I gave her at the time, I saw no more of her for some years, and thought that she was well. After about five years, however, she appeared before me again. She had at first lost her leucorrhœa. But it returned as soon as she omitted the practical remedies. It increased with every fresh menstruation, and latterly became more copious and abundant. General emaciation and great weakness came on, and she got very much more nervously irritable. But all these evils would not have made the patient seek medical advice a second time, had not two symptoms appeared which caused her the utmost alarm. Very violent dragging pains in the region of the womb gradually made their appearance, returning even in the night, and a change in the discharge followed of such a nature, that in the place of the milky, slimy secretion, there appeared a dirty, brownish, bloody, exceedingly offensive discharge, which soiled the linen day and night in great quantities. From these symptoms, I diagnosed that condition of the organ which is peculiar to uterine cancer. The result of the examination was happily somewhat better, though still very alarming. I found the ulcerations on the lips of the cervix hardly any larger than they were five years before; but at isolated spots, especially at the lateral angles, there were little cock's comb-shaped excrescences, and in the canal, from which a copious quantity of that smeary, stinking secretion escaped, one could see and feel that the mucous membrane was thickly beset with growths of various sizes and shapes, mostly pretty hard and tender to the touch, many breaking down under the weight of the finger even, causing a pretty copious hæmorrhage. This lady was, altogether, cauterised six times with chromic acid; a glass rod armed with it was pushed up the canal of the cervix as far as the internal os. After the sixth time, when I had, perhaps, gone somewhat deep, a severe traumatic affection of the uterus and its appendages followed, but she recovered in the course of three weeks.

Nevertheless, when the patient left her bed the uterus was healthy, the cervix closed, the abnormal secretion had disappeared, and the patient afterwards fully recovered her former healthy appearance. No relapse has taken place up to the present time, after an interval of five years. If we reflect upon these cases, we cannot deny that we ought not to blame women for their extreme anxiety lest their long-continued leucorrhœa should end in cancer. It is evident that special consideration and care are necessary at the commencement of these cases of cervical catarrh with ulcerations, in which the patient belongs to a family in which cancer has shown itself, for cancer is exactly the same, in whatever organ it occurs. It is a well-known fact that inherited cancer may establish itself in quite another part of the body to what it did in the forefather; for example, cancer of the liver in the grandfather may be followed by uterine cancer in the grandchild.

The uncommonly frequent occurrence of diseases of the cervix makes it very desirable that we should be able to discover the causes of the same, as quickly and safely as we are able to learn their site and peculiarities with the speculum. But most patients come under treatment at so late a period of the disease that it is not possible, in every case, to trace the origin of the mischief from its commencement. I can, however, give you the following as the general causes of cervical catarrh.

In by far the greater number of cases, pregnancy and labour, though quite regular in their course, afford us a cause for the commencement of this disease. We have very often examined persons in the last months of pregnancy in these wards. You recollect how often we have found the thick, puffy, spongy lips of the cervix so eroded, that we looked into a canal surrounded by raw walls. We can very well conceive that this exfoliation of the epithelium is the result of the spongy, hyperæmic condition of the mucous membrane of the cervix, as we know that the copious creamy discharge which comes away from the vagina of every woman during the latter weeks of pregnancy, is nothing more than a very great exfoliation of the vaginal epithelium, following upon its spongy, hyperæmic condition. These quite superficial ulcerations of the mucous membrane of the cervical canal, which so often occur that one is almost inclined to consider them as physiological, generally heal up very quickly after the confinement, and without any injurious effects, provided the labour is simple and not unusually prolonged, and if the necessary rest and nursing is insisted on. Now, imagine a case of labour in which the amniotic fluid comes away prematurely, before the os uteri has sufficiently opened. Think how the head, pressing for hours directly upon the spot deprived of its epithelium, bursts open the anterior lip of the womb; how the latter is squeezed and crushed against the anterior wall of the pelvis as the head descends, and you will not be surprised that, under such conditions, these ulcerations remain for a long time with little tendency to heal, even if partial gangrene does not follow. Think further of those cases in which the cervix is deeply torn by the forcible passage of the head through the lower portion of the uterus, and you have in addition a cause for long-continued suppuration which must necessarily affect these superficial ulcers. Now, if such persons have the good fortune and comfort of a fairly good lying-in time, the places on the lips heal up well, and so do those on the lateral angles of the os, without special treatment; and though their progress may be slow, no ulceration and no catarrh of the cervix remains behind. If, however, these patients leave their bed too soon, or go back to heavy work too early, so that the uterus gets irritated and dragged down, or if the necessary cleanliness is wanting after the confinement, or if, owing to incomplete expulsion of the decidua or blood coagula, or, in consequence of the retention of membranes (Eihautresten), these persons have the misfortune to suffer from an ichorous discharge from the uterine cavity, the ulcerations will get deeper, and it will require a very long time, even with care and rest, for healing to take place; whilst with neglect of the necessary dietetical precautions after the confinement, the ulcers and cervical catarrh become permanent.

Consequently, Gentlemen, you have the most frequent causes for the origin of cervical catarrh in pregnancy, labour, and the childbed. Apart from the childbed we find extremely various causes—partly mechanical and chemical irritants, and partly constitutional diseases. Amongst the first, the mechanical irritant, I consider masturbation to be one of the most frequent. It is self-evident that in masturbation by contact with the finger or with instruments, a partial inflammation of the vaginal portion of the uterus, together with catarrh and abrasion of the epithelium may—and, indeed, very often does —occur. Moreover, it is very probable that mechanical injuries give rise not only to catarrh, but also to the excessive irritability and hyperæmia of the uterine nerves and spinal cord, which manifest themselves, on the one hand, in relaxation of the muscular walls of the uterus and vagina, and, on the other, in hyper-secretion, in which it appears the cervix specially partakes. We have exactly the parallel of this in men, in whom we find a relaxation of the scrotum, and the accompanying hyper-secretion of the prostate and testicle. Just as prejudicial, as a source of cervical catarrh, as masturbation, and, moreover, bringing about its result in a very similar manner, is the constant employment of the sewing machine. It is, indeed, a very common occurrence for women who are somewhat weakly not to be able to bear the employment of the sewing machine for more than a few weeks, even though it is only a question of the wants of their own family. Workwomen in shops, who work from morn till night at their machines, get ill very often in a few weeks. We see these girls get doubled up, pallid, and feverish, and, finally, they feel so weak that they are obliged to give up the work. If one examines these women, one finds the vestibulum and the entrance of the vagina reddened, the clitoris often somewhat swelled, a copious vaginal secretion, and a somewhat sensitive mucous membrane; in all there is great sensitiveness of the cervix, and a catarrh, and, if the mischief has existed for a long time, there is also ulceration. I believe that the mechanical rubbing of the labiæ one against the other and against the clitoris by the to-and-fro movement of the foot on the treadle acts very similarly to an intentional masturbation, owing to the superficial inflammation of the mucous surfaces, which rub one against the other, and owing to the irritation of the nerves; and I am convinced that the headache, giddiness, and nervousness to which these workwomen are perpetually subject, and which they always attribute to the hum of their machines are certainly caused by the above-mentioned irritation of the nervous system. A simple comparison of the machines with regard to their different constructions shows this. It is no doubt possible that it may be the perpetual hum which each machine causes; that it may be the haste with which these women work their machines, whatever be their construction; or that it may be the quick alternation of the work on the same which first excites and afterwards depresses them and makes them nervously irritable; but facts show that the hand machines which buzz and rattle as much as the treadle machines are much less liable to lead to similar diseases, and never give rise to catarrh of the genital passages. The most dangerous of all, as far as my experience with these women has gone hitherto—and I can easily see the explanation of it, according to my view—is the single-footed machine, though the two-footed machine is likewise decidedly dangerous for weakly women, and for many hours employment daily.

But you must not think that local, direct, external injuries (insults) are the only causes of cervical catarrh. In the course of years the conviction has been forced upon me, which you may possibly remember in your practice, that even general constitutional diseases may find a home in the portio vaginalis. Not infrequently young girls are brought to me with excessive cervical catarrh; there is glassy discharge from and a knobby swelling of the portio vaginalis, whilst the body of the uterus is not enlarged, and the vagina is healthy. No mechanical cause can be assigned in these cases, there has been no anterior acute inflammation of the uterus, the malady has developed slowly and steadily whilst the patient was in moderately good health. But we discover that the patient was deeply scrofulous as a child, and certain tokens, such as swelling of the glands of the neck, scars of ulcers, nasal catarrh, &c., sufficiently indicate that the scrofulous state of things still exists. So many cases of this kind have occurred to me, and I have so carefully traced out the cause of the catarrh in them, that I have, after a careful exclusion of all the remaining causes, come to the conclusion that scrofulosis may by itself call forth the local mischief in the cervix; and I am also inclined to consider the mucous membrane of the cervix as one of the sites for the localisation of scrofulosis, just as the mucous membrane of the nose, the eyes, and the ears is. I am confirmed in this opinion by the circumstance that the same symptoms make their appearance as frequently in another nearly related disease, viz. tuberculosis: but hitherto I have not had sufficient opportunity to examine tuberculous girls. I have only made these observations on tuberculous women who had had children, and on this account I cannot decide whether these catarrhs had developed purely as the result of tuberculosis. So much is certain however, that no catarrh of the cervix is so persistent and so intractable under all local remedies as that of the tuberculous woman. It has also been asserted that the morbid condition of the blood in a chlorotic person may give rise to catarrh of the uterus generally, and especially to catarrh of the cervix. Scanzoni explains the origin of these catarrhs in the following manner: he thinks that the muscular weakness of the heart of the chlorotic is not equal to driving blood with sufficient force and in sufficient quantity to the ovaries and uterus in order to give rise immediately to a vigorous, quickly compensating menstruation. There will, therefore, mostly only be a feeble, but so much the longer continued congestion of the genital system, and the prolongation of this often brings one menstruation quite close to another. The consequence of this may be chronic "Anschoppung" of the uterus, which manifests itself in the enlargement of the whole organ and the hypersecretion of the mucous membrane. But if you will only accurately inquire into and trouble yourself about the commencement and first symptoms of the chlorosis, you will find that it is a bad sign as regards the catarrh when the first causes of the chlorosis are of an unusually heterogeneous nature. In many girls, indeed, chlorosis is evidently the consequence of masturbation, and the cervical catarrh is easily explained in the same manner: both are the consequences of the same bad habit. In many others the chlorosis has its foundation in an inherited tendency to tuberculosis, or in scrofulosis continuing from childhood, which I have before endeavoured to prove to you may give rise to catarrh of the genital organs, and especially of the cervix. In these chloroses, which I consider as primary diseases of the blood, *sui generis*, I have never been able to discover more than that behind a certainly often narrow, but usually not tense, easily distensible hymen, a loose-walled vagina was to be found, upon which rested an exceedingly moveable, very lax, often bent uterus, that is to say, a uterus with collapse of relaxation (Erschlaffungs-collapsus). Catarrh I found mostly only in the vagina, very seldom in the corpus uteri, sometimes, but seldom, in the cervix; a catarrh which, in the general collapse of the genitals, is easily explained by the rubbing of the relaxed mucous membranes one against the other, and especially of the cervix against the vaginal mucous membrane: these rubbings are brought about by the movements of the patient, and by the pressure of the intestines during the to-and-fro and lateral movements of the uterus.

That peculiar form of genital catarrh to which I have given the name of Vaginitis exulcerans adhæsiva is best included amongst these catarrhs which proceed from constitutional causes, because it mostly accompanies general illnesses, and is perhaps dependent upon them. This very copious leucorrhœa which excoriates the labiæ and even the thighs is more especially found in women who have passed the procreative period and who exhibit a tendency to liver disease and arthritis; it is accompanied by long-continued general symptoms, such as malaise, eructations, loss of appetite, constipation, very foul urine with an abundant sediment, and regularly by nervousness. When we employ the speculum, we find the introitus vaginæ and the lower two-thirds of the vagina somewhat reddened and covered with secretion, but not otherwise changed.

The upper third of the vagina which surrounds the portio vaginalis, and the portio vaginalis itself are, on the contrary, very much inflamed, though the inflammation is limited to the mucous membrane. The entire mucous coat of the portio vaginalis as well as the mucous membrane of the vagina where it surrounds and touches the former is of a deep dark-red colour, deprived of its epithelium, and covered with a sticky, thickish, often somewhat purulent bloody secretion. If this inflammation continues for a long time, the portio vaginalis and the vagina invariably get glued together in the whole circumference of the former; they may even grow together, completely extinguishing this portion of the vaginal cavity and concealing the portio vaginalis. The body of the uterus in this form of disease, as in the before-mentioned, is generally quite intact, not thickened, not painful, without perceptible catarrh; although it cannot be denied that when these diseases of the cervix continue very long, an extension of the mischief to the mucous membrane of the body of the uterus may take place.

If you now look back to the long list of diseases which attack the cervix, you will wonder wherein lies the anatomical reason that such a small portion of the uterus, a part so unimportant with regard to the highest functions of the woman and so little sensitive to the strongest irritants, should be so frequently, so obstinately, and so dangerously attacked by disease. But you will easily see the reason when you call to mind what I have told you before, viz. that the cervix takes a particularly passive part in the process of labour, that it is exposed to the most violent stretchings, often to pullings, crushings, and tearings during the passage of the head, that it contracts much less quickly and re-forms itself much more slowly than the corpus uteri, owing to its being so much less muscular, that, except in the puerperous woman, the cervix, owing to its anatomical position, is much more exposed to direct injuries in the vagina than the corpus uteri. With regard to the obstinacy of the mischief in the cervix, however, the anatomical structure of the mucous membrane will give you the best explanation, viz. its numerous folds, which, when swelled, so easily lead to a stagnation of the secretion, and its richness in glands and easily hypertrophied papillæ. Consider further, that the cervix, just like the corpus uteri, takes part in the menstrual congestion, that an aggravation of the already existing mischief, of some days' duration, must occur every month in consequence of this congestion.

The corpus uteri does not offer such favourable conditions for the origin of a catarrh. Its mucous membrane is smooth throughout, easily stretched, and is only separated from the muscular wall by a thin stratum of submucous tissue, and its buried position protects it from external injuries. Corresponding to these anatomical peculiarities, pure catarrhs of the body, compared to those of the cervix, are proportionally infrequent. Many physicians believe that they occur with very great infrequency. Nevertheless, I believe they occur more frequently, and that these catarrhs have not been noticed in the practice of such physicians because they have not known how to recognise them, for their diagnosis is not made so easily and quickly and with such simple means as is the case with catarrh of the cervix. The symptoms are very little characteristic. Women come to us who are troubled with an exceedingly watery often very copious discharge, though they do not complain of any special local mischief. If the disease has existed for a very long time, however, they feel weak and languid; they are nervous; they look pale; and, earlier or later, they suffer from stomach troubles. Menstruation may be quite normal, although it is usually too abundant at the commencement of the mischief; but, after a twelvemonth's continuance of the catarrh, it usually gets sparing. The power of conception entirely disappears after a time, probably because the epithelium of the diseased mucous membrane, as is usually the case with long-continued catarrhs, alters its character. The ciliated epithelium disappears; in its place we get cylindrical, later, polymorphous cells closely resembling the pavement epithelium. The mucous membrane itself atrophies; its glands disappear; it becomes more like a thin stratum of interstitial tissue. Except the sterility however, the only striking characteristic symptom, even to the women, is the frequently abundant morbid secretion. Whether this secretion proceeds altogether, or only chiefly from the cavity of the corpus uteri, we cannot discover either with the finger or the speculum. Microscopical and chemical examination give us some help; and the examination with the sound, by which we find that the mucous membrane in recent cases is puffy and very tender to the pressure of the knob of the sound, enlightens us also. These, however, are no safe guides. When only a moderately copious catarrh exists, we get the most direct and authentic knowledge as to the site of the catarrh by means of Braun's uterine injection syringe, which must be introduced into the cavity of the uterus with the piston pressed down so that when it is drawn up the syringe may suck up the contents of the cavity of the body. The fact of having withdrawn secretion direct from the cavity of the body is quite enough to make the diagnosis certain. Additional evidence is got when we find the secretion is thin, of a yellowish red colour, and that it contains ciliated epithelium. But the latter may be wanting, and then, trusting to the other facts, we must conclude that the catarrh has existed for a very long time, and that, under its influence, the ciliated epithelium has disappeared. Thus you possess, as you see, in these particulars a criterion for the estimation of the age of the catarrh. I recollect a lady from St. Petersburg, who, when about thirty-five years old, got leucorrhœa whilst nursing her third child, her confinements having been quite regular, but following close upon one another. The discharge got more abundant week by week. She took the child from the breast, but it made no difference. The most varied means which the physicians of her native town ordered were without result. I saw her three years after the commencement of the catarrh. It was an instructive case of catarrh of the body. She was pale and looked more than her age. The discharge was so copious that her diapers were continually drenched. This enormous discharge was so repulsive to the husband that he separated from her conjugally and finally charged her to seek medical advice in Germany and not to return home until she was fully recovered. After having carefully mopped up the secretion from the healthy vagina, I convinced myself, by means of the suction experiment with the syringe, that this lady's leucorrhœa originated solely in the cavity of the corpus uteri, the walls of which exhibited more tenderness when touched with the tube of the syringe, especially at the fundus, than is usually the case with the healthy uterus. The secretion was watery and thin. It contained no ciliated cells and but few cylindrical ones; the greater number of the cells much resembled those of pavement epithelium. The lady went home after quite a long-continued cure, and has since remained quite free from her catarrh. Whether she has again conceived I know not.

In many cases it is impossible to give the causes of this catarrh of the body, though we have learnt one in the foregoing case.

A series of cases quite analogous to the one just quoted which have come under my observation have forced me to the conviction that a great number of catarrhs of the body, like the before-mentioned case for example, originate in undue nursing. It may be that the women undertake the nursing when they are not fitted for it at all, or that they nurse for too long a time, or too often owing to their confinements occurring so quickly one after the other. Mostly, at least in quite half the cases, really copious hæmorrhages precede these profuse, serous, slimy discharges. One may conclude from these circumstances that, in these cases, the hyper-secretion owes its origin to a relaxation of the integument of the uterus, with simultaneous irritable condition of the spinal cord, similar to that caused by masturbation, only that the nervous track is different; and, indeed, the results of the examination of the uterus correspond throughout to this view. The genital organs are relaxed, hanging down; the uterus is small, soft, and easily bent.—In many other cases the catarrhs of the mucous membrane of the body are evidently pure catarrhs from obstruction, the result of narrowings at the internal os; similar to the catarrhs which occur in consequence of flexions or senile atrophy, or in consequence of the before-mentioned constrictions of the external os as the result of cicatricial contraction. Many cases begin in childbed, in consequence of defective puerperal re-formation of the body of the uterus, imperfect reproduction of the mucous membrane, endometritis, or defective contraction of the uterus. More rarely this catarrh originates after abortion, owing to some of the decidua having been left behind; and when this is the case, we almost invariably get intercurrent, painful, often very copious hæmorrhages. But we find catarrhs of a similar nature in the presence of new growths of various kinds on or under the mucous membrane of the body. The discharge is most copious with the diffuse sarcomatous growths, but extremely so in the presence of polypi, whatever be their form. But they are sometimes the result of little circumscribed hypertrophies of the mucous membrane, the presence of which during life cannot be recognised with certainty until we have excluded the other larger tumours; but after death they are not infrequently found as little, usually bluish red, cone-shaped excrescences on the mucous membrane.

All known catarrhs of the mucous membrane of the corpus uteri may gradually extend up into the mucous membrane of the Fallopian tubes. But we only learn this from pathological anatomy. The symptoms of the disease, and even examinations of the patients, do not inform us on this point. If, owing to tubular catarrh, a bladdery distension of the Fallopian tubes does take place, forming the so-called Hydrosalpinx, and that consequently a palpable swelling can be felt by a bimanual examination, we can hardly decide with certainty whether we have really to do with this affection, or an ovarian cyst, or an exudation into the parametrium, etc.

Therefore, Gentlemen, you will recognise catarrhs of the uterus. I have given the description of them before that of the vaginal catarrh, because I consider them to be the more important in their consequences. As you have seen, they undermine the constitution, they destroy the power of conception, and they may lead to carcinoma. Catarrh of the *vagina* behaves otherwise; to the description of this we will now pass. Women may go about for years with a leucorrhœa originating in the mucous membrane of the vagina, without its injuring their health in the least. But when the secretion is very copious they suffer from the loss of the fluid from the body. Neither near nor remote affections of the uterine nerves make their appearance as they do in cervical catarrh, nor do those deeper local mischiefs.

Pure catarrh of the vagina is, however, a rare disease; but in combination with a primary uterine catarrh, especially with that which proceeds from the cervix, vaginal catarrh is, on the contrary, uncommonly frequent; resulting partly from extension of the catarrhal mischief to the mucous surface of the vagina, and partly from the secretion running over and irritating the mucous membrane; very often it is a joint symptom of a disease affecting the whole mucous membrane of the genital tract.

The aged patient from whose vagina we yesterday removed a Mayer's pessary which had been used too long offers a very instructive example. From her genitals trickled continually a sticky secretion which quite soaked her linen. After we had removed the stinking instrument, which had become rough on its upper surface from long use, we found the mucous membrane spongy, deprived of its epithelium in isolated spots, and everywhere brightly reddened. Especially posteriorly, close behind the vaginal portion of the uterus, the ring had galled the mucous membrane to such an extent that at this spot there was a dark red, easily bleeding, somewhat granulated, longish, ulcerated spot. The ring had, owing to its excessively long employment, acted injuriously in a twofold manner; it had been gradually corroded by the sour vaginal secretion, had lost its smooth upper surface, and had become simply a mechanical irritant owing to its rough surface. But also owing to the stagnation of the secretion above the ring, and the retention of a mass of effete epithelial scales mixed with portions of interrupted menstrual discharge which the upper border of the ring kept back, there arose gradually a putrid mass decomposing in consequence of its close contact with the atmospheric air, which also had a chemically corrosive effect upon the already raw mucous membrane.—But the commonest mechanical and chemical irritants are those which give rise to pure primary vaginal catarrh, and, as a rule, to pure primary vaginal catarrh alone. You recollect what an extremely profuse catarrh arises in consequence of the mechanical irritation of the prolapsed portion of the vagina in prolapsus vaginæ, what a continual discharge of milky fluid is gradually called forth from the vagina of women of the town, in consequence of too frequent cohabitation. And let me add, that I have seen profuse catarrhs, sometimes with simultaneous or closely following catarrh of the bladder, not infrequently originate in old women, whose rima has opened in consequence of a shrinking of the labiæ, so that the dust has got into the vagina in the summer, and the cold air in the winter; and these catarrhs would not disappear until I had had a broad T-shaped bandage applied, which made up for the deficient closure of the vagina, and warded off the external mischief. Amongst the chemical irritants you have to reckon the effect of the decomposed urine in cases of genito-vesical fistula, but, before all, gonorrhœal infection.

You will find that pure catarrhs of the introitus vagina and the vestibulum originate in the same causes, viz. uncleanliness, dust, fæces, overflowing and decomposing urine, and ascarides which have crept from the anus over the perinæum to the labiæ; the latter is an especially frequent cause of catarrh in children, But sometimes purely mechanical causes are brought to bear upon these catarrhs. I will relate to you, as an example, a case of extremely profuse catarrh of the introitus, originating in thi manner in a young sterile woman.

This lady, who was somewhat corpulent, was so plagued by the copious, slimy, purulent, excoriating discharge, that at last walking was painful to her. The labia majora, the fork of the thighs, even the upper portion of the thighs themselves, in spite of the most scrupulous cleanliness, were deep dark-red and shining, and the thin skin at isolated spots was raw and cracked, so that it bled easily. On an examination, the finger passed into the introitus without difficulty, and seemed to reach a somewhat too short vagina, which got narrow above, but at the end of which a portio vaginalis was not to be reached. This circumstance led to an exact ocular inspection. It was then quite clear that the tough hymen of this woman, who had been married two years, had not been ruptured, it had only been very much stretched, and had formed a kind of cap, close under the portio vaginalis. The causes of the catarrh here were evidently the forcible attempts to break through the hymen during cohabitation. But this lady had suffered specially badly from leucorrhœa because she was so very fat; in fat persons, discharges and sores may arise from nothing but the usual rubbing of the surfaces of the skin one against the other. We split up the hymen completely, and drew the flaps on one side. After that we could plainly see that the catarrh was quite limited to the introitus and the hymen. In another patient, the wife of an innkeeper, a most profuse, purulent catarrh of the vestibulum originated in numerous new growths which had grown to a considerable size on the external genitals. From the labia minora, the clitoris, and the labial commissures ragged, glandular, cylindrical, pendulous tumours, from 6 to 8ctm. in length, and 2ctm. in breadth, grew irregularly, and resembled molluscum simplex in their histological structure. The discharge stopped when these tumours had been completely removed. The woman attributed her illness to her husband, who very often got tipsy of an evening, and worried her with rough manipulations and very frequent coïtus. It is conceivable that her explanation of the origin of the mischief is correct.

Swelling of Bartolini's glands is not an uncommon accompanying symptom of catarrh of the introitus. These glands, as you know, lie inside the labia majora, and send their secretory ducts straight to the vagina; the ducts open in front of the hymen. These secretory ducts are particularly liable to get stopped up and prevent the discharge of the secretion where they are somewhat narrow; this may occur partly through stoppage with dust and shreds of epithelium, and partly through swelling of the secretory ducts resulting from mechanical irritation. Owing to the stagnation of the secretion, and the irritation and inflammation of the glands and the surrounding tissues, we get, sooner or later, quite large, roundish tumours, which, if left alone, always run on into suppuration; they obstruct the introitus vaginæ, and owing to their spreading out externally, they hinder the movement of the leg on the corresponding side, and owing to their swelling internally, coïtion is impossible; and, let me add, it is an affection which does not always require the lancet or knife, it can very often, in its first stage, be cured by the catheterisation of the secretory duct with a thin probe. A further accompanying symptom of catarrh of the introitus (though it may be a self-existent mischief, and the catarrh secondary to it) is the following peculiar affection of the urethra; the orificium urethræ gets very spongy, thick, swollen, and deep, dark-bluish red; from it project some excrescences which, owing to their broad bases and the polypoid and cock's comb-like way in which they spread about, obstruct the excretory duct, and which, though varying, do not usually exceed the size of a cherry; the mucous membrane is hypertrophied in parts, especially over the papillary bodies, where it is usually hyperæmic and bleeds on the slightest injury of the epithelium. There is very often an abundant, purulent, stinking discharge, with many painful troubles. The urine is passed with difficulty, often with cramp of the bladder, and causes great smarting as it passes over the raw spots of the introitus. Walking is only possible with the legs spread out; if it is persisted in, inflammation arises in the neighbourhood, with swelling of the labia and clitoris, even to the formation of ulcers. Coïtus is in most cases, even in the slightest, simply impossible, partly because the introitus vaginæ and its neighbourhood is too tender to permit the introduction of the penis, partly because the urethra, as far even as the bladder, is usually so thickened and swelled that it forms a very tender, puffy swelling which considerably lessens the vaginal aperture. Women have confessed to me that when in spite of this mischief they have indulged in coïtus, they have fainted away with the pain, that they afterwards bled for hours, and then, owing to the inflammatory swelling of the genitals, they have been obliged to keep their bed for days. It might appear to you that you had to do with gonorrhœal infection in this disease. But this is not usually the case. It occurs with special frequency in old, decrepid women, and in virgins after the cessation of the menses.

Now, some few words with regard to the *acute* genital catarrh of women. It is, on the whole, seldom primary; it is usually an aggravation of the before-mentioned chronic catarrh; the result of very various evils which may be either external or internal, but which must nevertheless be of quite a severe nature. Primary acute catarrh does occur, however. We see it appear after sudden severe chills, especially during menstruation; and it is then usually spread over the mucous membrane of the whole genital tract, and is often followed by diffuse metritis and parametritis. We also sometimes get acute catarrh as one of the more troublesome symptoms in typhus, especially in T. exanthematicus, with an extremely profuse, thick, purulent secretion, which inflames and ulcerates the labiæ and causes them to swell; a similar state of things occurs in variola and scarlatina; and in all these diseases it may be followed by cicatricial contraction and deformity of the vagina. The acute catarrh which results from the growth of the before-mentioned fungoid growths generally runs a much less intense course, with less swelling and smarting, although with a troublesome feeling of heat and itching in the genitals. But acute gonorrhœal catarrh most frequently give us an illustration of an acute inflammation of the female genital passages. Picture to yourselves a patient who has only noticed this affection some three or four days; you find, on inspection, great swelling of the labia majora, reddening, loss of epithelium, œdematous swelling of the labia minora; the mucous membrane of the vagina, from the introitus to the vaginal arch, is covered with blood-tinged, thin, purulent secretion; the mucous membrane itself is œdematous; in many places it appears excoriated through loss of epithelium. The introduction of the mirror is attended by much pain, it is often impossible to get it in; the urethra is, even now, often thick and swelled, though it may not always be so throughout; its orifice is œdematous and covered with pus and blood. The patient complains to you of smarting and burning in the womb, difficulty in walking, and of burning and stinging whilst making water. These women take no care of themselves, and, indeed, in the pursuit of their occupation as prostitutes, they often increase their malady by indulgence in spirits or dancing, and sometimes even by the continued renewal of sexual intercourse; in consequence of this they often suffer from an extension of the inflammation to the uterine mucous membrane, with the symptoms of a moderately acute metritis, which may even extend to the ovaries through the Fallopian tubes, and produce distinctly perceptible symptoms of acute ovaritis, such as pain in the corresponding hypogastric and lumbar regions and impossibility of standing upright or moving about; and a very sensitive, swelled, not easily moveable ovary can be felt from the vagina. It is evident from this that catarrho-gonorrhœal inflammation of the female genital organs may, owing to great neglect, take a very similar course to what this catarrh does in men; in whom it may, under similarly unfavourable conditions, extend to the prostate, the seminal vesicles, the epididymis, and the testicle.

Acute gonorrhœal catarrh has, moreover, a much greater tendency to take a chronic course in women than it has in men, and to drag itself on through months or even through years, evidently because the mucous surfaces are more extensive, and provided with folds. To distinguish this from the non-infective catarrh by an examination is scarcely possible, unless pointed condylomata and catarrhal affection of the urethra indicate which disease we have to do with. I will take this opportunity of mentioning an observation I have made, which, if it should be well confirmed, would be of great interest, and well worthy of notice. It has appeared to me that the papillary tumours which I have already described to you as sometimes found at the external orifice of the uterus, and which may lead finally to carcinoma, can most frequently be traced to gonorrhœal infection; this conviction has been more particularly forced upon me by what I have observed in some women who were married whilst their husbands were suffering from slight gonorrhœa, or whose husbands, by their indiscretion after wedlock, had caught a virulent catarrh, and had had intercourse with them before the disease had quite disappeared. This was strikingly exemplified in the case of a woman, now forty-six years old, and the mother of five children. This woman up to her forty-fourth year was healthy, strong, menstruated regularly, and quite free from leucorrhœa; in June, 1869, she remarked the sudden appearance of a slimy, purulent, but moderate white discharge, which gave rise to some pruritus and sores on the labia majora. In the October of the same year she sought medical advice, for she found that washing and injections with water did not remove the mischief. In addition to a moderate vaginal catarrh and a somewhat swollen uterus, I found quite a florid, cervical catarrh, with ulcerations of the lips, and small papillary growths in the cervix, which would not yield to cauterisation with solid argentic nitrate, and could only be removed by the energetic employment of the red-hot iron. The husband confessed to me that in May, 1869, he had caught a moderately intense gonorrhœa, for the removal of which he had had medical advice, but that before the entire disappearance of the discharge he had had connection with his wife. Not many weeks after, I found a rough excrescence on the portio supra-vaginalis of this patient's uterus, the cervix of which had fully healed up and got quite smooth; the growth was situated in front of, and below Douglas' pouch, and projected into the rectum, causing much nocturnal pain and great difficulty at stool. From these symptoms, and their insidious commencement at the spot which a short time before had been cleansed of its papillary growths, I diagnosed carcinoma.

I am acquainted with three other similar cases in which moderately quickly growing papillary tumours, which only yielded to the hot iron, originated in gonorrhœal infection. If these cases are not the result of a coincidence, but of a causative connection between gonorrhœa and papillary tumours, as indeed is rendered probable by the analogy with the condylomatous growths on the external genitals, then it would appear that the indiscretion of the husband, who, whilst suffering from the remains of a gonorrhœa, renews intercourse with his wife, is visited very heavily on the woman; and then the old, almost forgotten, lesson in the ætiology of uterine cancer—

that it may originate in a gonorrhœa—would again be worth something.

Just as polymorphous and manifold as the malady is which you have learnt to recognise under the name of leucorrhœa, but which we call catarrh of the female genital organs, so is its treatment simple. It consists generally in a judicious employment of caustics and astringents, of which only the form of employment and the greater or less concentration makes the essential difference. This must nevertheless be premised. There are cases in which quite a complicated treatment is necessary, and least of all must you follow in that old beaten track of those physicians who, without reflection, and without submitting the genital organs to an exact examination, prescribe astringent injections for the vagina for every woman who complains of leucorrhœa, and never do more than question themselves or others whether they ought to confide in argentic nitrate or copper more than in acetified clay or tannin, etc. I have no doubt you recollect the case which, but a few weeks back, demonstrated so strikingly the great evil and uselessness of one of these remedies which was only directed at the symptom of the chronic discharge. I refer to that little, weakly blonde in the second bed in the second room, who, at her youthful age, is already the mother of five children, and has had three abortions besides. She suffered after the last abortion, which took place seven months ago, from severe leucorrhœa without pain, but with the sequelæ of general weakness and wasting. They had all the time given her ferruginous preparations in abundance, and had made her use injections of alum. But the iron did not relieve her; it only increased the menstruation, and the injections finally gave rise to an irritable condition of the uterus and bladder, without removing the discharge. We made an exact examination of the uterus, and found it enlarged and very spongy. The sound glided into a wide, loose cavity, and as one moved it too and fro, it caused pain and hæmorrhage as soon as the knob of the sound touched certain somewhat uneven but soft and sensitive spots at the fundus. This state of things induced us to dilate the cavity of the uterus with compressed sponge; and when this had been accomplished, we could feel with the tip of the index-finger that there was an uneven, somewhat ragged mass at the fundus uteri, about the size of a bean, projecting from the otherwise healthy mucous membrane. We immediately removed this mass with the finger-nail, until only the smooth uterine surface was to be felt, and then injected into the empty cavity a small quantity of a dilute solution of the sesquichloride of iron. After that, the discharge as good as disappeared; and the patient, since this little operation was performed, has recovered delightfully, as you have seen. Menstruation has not yet made its appearance. Of what benefit could any astringent injections into the vagina have been in this case if one had not quickly put an end to the real cause of the mischief? I have purposely recalled this case to your memory, because it at the same time gives you a general idea as to the mode of treatment of catarrh of the corpus uteri, which we shall mention next, viz. the direct local treatment of the diseased mucous membrane, and the removal of any foreign bodies which may be present. You already know the nature of the foreign bodies which one may find on the uterine mucous membrane after one has made the uterus accessible to direct examination by dilatation with compressed sponge. There is a tendency to the formation of the fibrinous polypi, as in the example just given; there are the polypoid excrescences from the degeneration of the glands; and there are simple hypertrophies of the mucous membrane. These usually not very extensive tumours can, as a rule, be successfully removed by the finger-nail, and, for my own part, I am hardly prepared to scrape out the cavity of the uterus with the instruments which are constructed for that purpose, such as Recamier's scraper, and the scraping instruments lately made in imitation of this in Berlin. Sometimes these instruments seriously injure the sound part of the uterus, whilst the diseased portion is left behind; consequently, if there is any roughness in their usage they may do mischief, without answering their purpose. It is not usual for the catarrh to continue after the removal of the foreign growth; but if it should do so, we must begin a regular course of local treatment.

We shall not stop to describe and exhaustively criticise every method of local treatment. I will only shortly mention, that I cannot give my support to the treatment in which solid substances are introduced into the cavity of the uterus. I consider it both painful and dangerous, and, at the same time, of uncertain benefit, to introduce solid lunar caustic, although only in little pieces, into the cavity of the body, and let it dissolve there. I consider the employment of plugs of tannin or alum, made up with gum-arabic, as no less disadvantageous, and the employment of vetches which have been soaked in astringents, and are intended for introduction with the sound, I look upon as nonsense. Whether Spiegelberg's plan of cauterising the cavity of the uterus by means of a porcelain-point in connection with a galvano-caustic apparatus is as free from danger as, according to the latest published results of this very experienced gynœcologist, they are shown to be effectual, time only will show. I consider that the employment of fluids, which should be injected into the cavity of the uterus with appropriate instruments, as the only treatment which is, with certain precautions, entirely free from danger.

Your own experiences as to the uses and safety of this procedure have hitherto been confined to the lying-in wards, where you have often seen its surprisingly quick and beneficial results in hæmorrhages after delivery, in weakening excoriating catarrhs, diphtheritis, and the ichor resulting from retained fœtal membranes and coagula. But I told you then that the first attempts which were made to treat the non-puerperal uterus in a similar manner were often followed by the most painful results, such as metritis, peritonitis, and death. It is only within the last decennium that we have discovered, as the direct result of our experience with the puerperal uterus, the safeguards which are necessary to make the injection safe, and, at the same time, remedial; these I will now shortly relate to you: but, at the same time, let me repeat that we are only speaking of the treatment of pure catarrh, and therefore only of that disease of the mucous membrane which occurs in the relaxed uterus after delivery or abortion, of that which occurs in flexions, the result of relaxation, and of that which occurs in the before-mentioned relaxations of the uterine walls; and that we are not speaking of the treatment of the secondary catarrhs which accompany the hypertrophied uterus, the uterus indurated by exudation, or even the uterus surrounded by exudations. Under the latter circumstances the injection is certainly not tolerated.

Always take warm fluid for the injection, otherwise you will get painful uterine colic. Further, you should never inject more than from ten to fifteen drops, and take, first of all, only a dilute solution of your remedy, until you have proved the sensitiveness of the uterus, and have learnt that a stronger means is necessary. In this way you may perhaps get so far as to inject the pure solution of the sesquichloride of iron or the undiluted tincture of iodine. Do not use any other fluids than these. They are the only ones I can recommend, except glycerine. Of the remaining caustics, some have been tried and found hurtful, whilst none of the others have been sufficiently tested. Glycerine is found to be the most useful in chronic relaxed swelling of the mucous membrane, such as specially occurs in the profuse catarrhs of old women and chlorotic girls. Its good effects are evidently due to its drying-up powers. But, as is quite right, the greatest aim of the advocates of uterine injection has always, of late years, been to provide a means for the quick and complete evacuation of the injected fluid. With this object, English physicians, as a rule, widen the uterus in its inferior portion by means of laminaria or compressed sponge, and consequently endeavour to place it under the same favourable condition as it is in puerperal females. Most German physicians have followed this example, with the exception of some few, of whom some inject a very few drops, from three to five or eight, which are left in the uterus, whilst others inject large quantities, for the outflow of which they provide double-tubed catheters. I do not think the last two methods sufficiently provide for all the dangers which accompany injections. Neither can I recommend the usual, so much extolled, English method of dilatation, for I do not think it is always free from danger; and it is in any case much more complicated, painful, and tedious than the method I use. I inject, according to the size of the cavity, from ten to twenty drops with a Braun's uterine syringe, let the syringe remain in the uterus for a moment, and then, as soon as I consider that the fluid has had sufficient time to act upon the diseased mucous membrane, I suck up the fluid again by drawing up the piston of the syringe. In order that I may withdraw all the fluid, I bring the canula of the syringe gradually towards the internal os as I draw up the piston. This method, which I have found not only simple and free from danger, but also quite sufficient, I have used with success for so long a time that I can recommend it to you as thoroughly to be depended on, and better adapted to its purpose than the dilatation with sponge-tents. A short comparison of the two methods will show you this very clearly.

After the fluid has been injected and drawn off, an operation which can be performed without preparation, the patient need not usually lie down longer than half an hour. After that she may go about again freely. After an interval of two or three days, during which smarting sensations in the lower abdomen hardly ever occur, the injection may be renewed. In order to prepare the uterus by means of the sponge-tent for the employment of the injection, the patient must, on the contrary, lie for hours, until the sponge-tent has sufficiently expanded, and then, perhaps, the injection may be performed. After the injection she must lie down again. Perhaps, however, one introduction of the sponge-tent does not suffice; then a very extensive dilatation must be reached if the internal os is intended to remain open a longer time, until the injection has flowed out again. Sometimes it has to be dilated not only once, but even a third time, before the operation can be performed with perfect safety. In this way from twenty-four to thirty-six hours are consumed about a single injection, during which the patient must keep her bed, and this has to be repeated every four, five, or seven days for some weeks, very numerous injections being necessary; —surely a worrying and painful procedure for the patient. The frequent introductions and withdrawals of the sponge-tents have a prejudicial effect upon the uterus, let alone the pain they cause. The mucous membrane of the cervix not only gets very much compressed, but also more or less destroyed, and the internal os is necessarily earlier or later irritated; and into this organ, with its mucous membrane injured and its walls inflamed, the injection is thrown. It is evident that, under these circumstances, even very weak solutions may be too strong, and, owing to the injections which are intended to heal the catarrh having to be repeated several times, we may, under some circumstances, get metritis; and, finally, we are never absolutely certain that this method fulfils its peculiar object, viz. that of securing a quick return of the injected fluid. For it is well known how quickly the internal os of an irritable uterus closes again; and this will take place much more quickly when the injection gives rise to an abnormal amount of irritation at the internal os. None of these inconveniences are attached to my method, as you see, and I therefore advise you to make use of it yourselves.

But you must not think that you are doing enough for every catarrh of the *corpus* uteri when you employ local treatment alone. This will only be the case where purely local causes come into play. As you are already aware from the foregoing cases, the causes of the catarrh are often rooted in constitutional mischiefs; these, as well as the simultaneously occurring derangements of digestion, sleep, activity of the skin, etc., are worthy of special attention. In these cases you will find that the correct general treatment is to be found in good therapeutic rules of medicine.

And this is of similar importance with regard to *cervical* catarrhs; indeed there are numerous catarrhs of the cervix which can only be removed by a careful general treatment, whilst energetic local interference may be quite injurious. This holds good, before all, for cervical catarrhs in conjunction with pulmonary tubercle. If you make use of lunar caustic, astringent injections, and preparations of glycerine in a woman whose lungs are even but little diseased, and have been so only for a very short time, you will probably increase the mischief; the cervix will swell, and the application of the caustic will become more and more painful, and therefore more obnoxious to the patient. But, in addition, the amount of the secretion which the ulcerated spots secretes gets no less, it may even increase. It is very apparent that under these conditions the local treatment of the uterus will do more harm than good, since it weakens the patient, and, owing to the lack of benefit from the operation, it still more depresses her already low spirits. You must therefore always very carefully examine the lungs of every patient who suffers from cervical catarrh and does not present a perfectly healthy appearance; and in any suspicious condition of the lungs, not to mention a diseased state, you should always limit the treatment of the uterus to purifying, mild, sedative measures, such as lukewarm injections, with a decoction of linseed or an infusion of chamomiles. Also with scrofulous patients, I advise you not to act too energetically, unless their constitution has been restored for a very long period by salt baths, iodine, or chalybeate springs; if this is not the case, you had better restrict yourselves to cleansing the genitals with tepid injections. But the local treatment of cervical catarrh consists essentially in the employment of astringents and caustics; these should generally be pretty concentrated, and should be applied both to the spongily degenerated lips of the cervix, and to the mucous membrane of the canal itself. The method of treatment here, however, is far more simple than it is with the body of the uterus; but even here there is a difference in the treatment, according as we have to do with virgins, or with women who have had children.

As I have before pointed out, the cervix of virgins is usually much tapered inferiorly, the orifice is often narrowed by cicatrices, and the canal is frequently obstructed by swelling of the mucous membrane, hypertrophy of the glands, and thickening of the secretion. The first indication here is that we should widen the canal so that the secretion may freely flow away and the diseased mucous membrane be exposed to direct treatment with caustics. For this purpose the employment of tents is insufficient. A few days after the removal of the tent, the cervix has again narrowed to its usual size, and in spite of new dilatation the old mischief always reappears. The only way to widen the canal permanently is to divide the walls of the cervix on both sides considerably with the knife. The method of division is sufficiently known to you. After this division, complete cicatrisation of the wound must be allowed to take place; it usually requires from fourteen days to three weeks; before that, cauterisations of the mucous membrane must not take place.

The jagged cervix, whether it be that widened by the just described incision, or that of the multipara, will stand the strongest caustic applied to its mucous membrane; nevertheless we must not go deeper than a little below the internal os. The cauterisation of this part itself is altogether to be deprecated, for it reacts to the slightest irritation most rapidly and sensitively, and by undervaluing this precaution, diffuse metritis and parametritis quickly make their appearance. The astringent or caustic one chooses depends partly upon the duration of the disease and the amount of degeneration which has taken place in the mucous membrane. On the whole, nevertheless, we avoid those caustics the effect of which we cannot compute, before all, therefore, the alkaline caustics. In slight, not long existing catarrhs, I can recommend to you the employment of tannin, plugs, which are made of tannic acid, with the slightest possible admixture of gum-arabic or glycerine. Of such plugs choose a piece 1ctm. in length, and introduce it with the index and middle finger of the right hand; or in case this proves too difficult for you, introduce it with a pair of bent polypus-forceps, after the speculum has been passed up, and then let the remedy gradually dissolve in the cervix. If you do not succeed in altering the tone of the mucous membrane with a tannin-plug, you should take a stick of lunar caustic, such as we have here for this purpose. It is a cylindrical stick of about 1ctm. in diameter, rounded into a knob at the end, and fastened below to a wooden stick by means of collodion. You push it up to the internal os, let it lie for a few seconds in the cervical canal, and then withdraw it. Similar cauterisations to these must be repeated every seven days. In those cases where papillary tumours, and hypertrophy of the glands and the mucous membrane in general already exist, even the lunar caustic does not suffice. I advise you in these cases to apply the hot iron very early. The more frequently I have to do with this disease the more I am convinced that this is the best way to proceed, and hitherto I have never had to complain of its too early employment, though formerly I was often reproached with unnecessary protraction of the disease, in consequence of having employed too weak a caustic. But where patients will not consent to the employment of the hot iron, I use as a substitute chloracetic acid. This acts almost as powerfully as the hot iron; it quickly makes a firm scab, and without advancing so incalculably far in depth as is the case with alkalies or chromic acid, as recommended by Sims. Its application is made with a glass rod; but it does not so quickly, safely, and uniformily act as the conical- or olive-shaped hot iron.

If the cervical canal has healed, as one easily learns by the disappearance of the swelling of the cervix, the breadth of its cavity, and the absence of the vitreous, purulent secretion, the ulcerations on the lips of the os usually cicatrise quickly; for it is evident they must get cauterised during the cauterisation of the canal. Should there be any ulcerations left behind, as is often the case owing to defective cleanliness and want of care, they must be lightly touched every five or seven days with solid argentic nitrate; and this must be done whether they are of a purely catarrhal nature, or are conjoined with hypertrophy of the glands. The papillary ulcers, on the contrary, usually bleed easily when lunar caustic is used, and heal better after chromic acid, tincture of iodine, sesquichloride of iron, or acid. pyrol. But do not continue too long with strong caustics; make a longer pause when the ulcerated surfaces begin to heal at the edges, and then take a milder remedy, such as washing the diseased spot through the speculum with a weak solution of argentic nitrate, a solution of copper, creosote, and water, or a solution of acetified alum, etc.

In reality the same means are used for the treatment of catarrh of the *vagina* and *vestibulum* as are used for cervical catarrh.

In acute gonorrhœa, or acute vaginal catarrh originating in any other way, after absolute rest on the back, warm mucilaginous injections, and, perhaps, warm sitz-baths, have done all that could be expected of them, we must use energetic caustics, which must be gradually reduced in strength. We can cut short an acute purulent catarrh most quickly in the following manner: we must pass a tubular speculum in deeply, and as we draw it slowly and gradually back, the whole mucous membrane, from the vaginal arch to the introitus, must be strongly cauterised step by step with a thick stick of lunar caustic. This is the way I commence the treatment of those profuse obstinate catarrhs of old women, as well as that of vaginitis adhæsiva, and the profuse catarrhs of the vestibulum. If the primary copious catarrh is removed, and only a thin, slightly purulent secretion proceeds from a somewhat relaxed mucous membrane, then I wash out the vaginal canal with the before-mentioned fluids every day, or every two or three days, as the case may be. In such diseases many physicians have a fancy for the employment of medicated sitz-baths, whilst the patients have to introduce a bath-speculum themselves. I decidedly disapprove of the employment of the bath-speculum, not only in the disease we have just spoken of, but altogether. For these patients, without mentioning that the introduction of such a speculum undoubtedly excites them in the same way as masturbation does, do themselves harm, if there should be any ulcerations on the cervix or in the vagina, by irritating and mechanically injuring the diseased spots.

I have seen not a few patients to whom the employment of the bath-speculum had been recommended, who, during and after each bath, had severe pain in the abdomen accompanied by hæmorrhage. But we can the more easily do without this instrument, which is constructed on such a vicious principle, because we possess a means whereby those patients who cannot have our continued personal attendance may carry on the treatment of their internal genital parts.

I know hardly any remedy which so quickly removes a simple chronic catarrh of the vagina, provided it is not complicated, as the tampon smeared with alum-ointment. Having had the tampon made about the size of a hen's egg of good wadding without knots, I have a portion of it rolled together hard and wound round in the middle with a strong thread, a long end of which is left for the after-removal of the tampon. This wadding is smeared all over with an ointment which consists of five parts of alum and thirty parts of fat. The women usually very quickly learn how to push such a tampon up to the vaginal arch with the finger, without the aid of the tampon-director recommended by Sims, which I consider as worthless as the bath-speculum. Side by side with the alum tampon in its good results stands the glycerine tampon, which Sims has brought into such extensive use. A ball of good wadding is soaked in chemically pure—but, as I have found the best, not perfectly dry—glycerine, which is then squeezed out of the tampon, and the tampon is pushed up as far as the vaginal arch. These glycerine tampons have appeared to me to be especially, but, at the same time, almost exclusively useful where it is a case of pure vaginal or cervical catarrh without ulceration, owing to defective involution of the genitals after confinement. The drying-up quality of the glycerine has a very good effect upon the tissues here. The tampons had better be used only at night-time. They cause a continuous, glutinous, copious discharge so long as they remain, and therefore keep the patient's linen continually wet if she go about with them in the daytime; which is not only disagreeable, but quite likely to give rise to chills.

You will see plainly enough that besides the foregoing remedies and methods of treatment there must be still numerous others which are more or less frequently found useful by other physicians in the treatment of catarrh of the female genitals.

It would lead us too far were I to relate them all, or even criticise them. I have only laid before you those therapeutic means which I have myself thoroughly proved by trial on a great number of patients, and have found worth recommendation; and upon these you may rely in your future practice.

If, however, in the course of time, you forget much of the little which I have told you with regard to this extensive subject of catarrh, you must never cease to bear in mind one thing—that you will never find a successful remedy for this disease which is so little thought about by many physicians, unless you make a very exact examination of your patient, a very searching inquiry as to her constitution, and, finally, an extensive examination of the whole genital canal and its nearest surroundings.

THE

TREATMENT OF FEVER* (PYREXIA).

BY

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UNTIL quite recently both physicians and laymen were almost unanimously of opinion that fever of itself was to be regarded as a rather favourable circumstance than otherwise. It was looked on as a salutary effort of nature, and it was believed that by means of the fever the body was freed from the noxious material of the disease, that, in fact, the fever was necessary to the recovery of the patient.

This view of the salutary effect of fever was supported by ancient authority. As far back as in the writings of Hippocrates we find the opinion repeatedly expressed, that in certain diseases the advent of fever is a favourable sign, and this opinion was entertained by most of the authors of antiquity. Asclepiades, indeed, is reported to have pronounced fever to have been his pre-eminent remedy.

This idea was further perfected and generalised in comparatively recent times. The formula of Thomas Campanella, that the object of fever was always to remove from the system the existing products of disease, was the foundation of the whole doctrine of fever. Besides this, some people regarded attacks of fever as conscious and voluntary efforts of the Archæus, who, in that manner, wished to free himself from the detrimental substance that had forced itself into the system (Van Helmont). Others designated fever more simply as "Instrumentum naturæ, quo partes impuras a puris secernat" (Sydenham); or else they called it "actum vitalem motorium, secretorium et excretorium, mediante quo præsentes quædam noxæ removeantur" (Stahl). A salutary effect in relation to other diseases was also presupposed. "Febris sæpe medicamenti virtutem exercet ratione aliorum morborum" (Boerhaave). And only a few decennaries ago the view was defended, that, in the proper application of fever lay the whole secret of medicine and surgery (Sobernheim). Particularly in respect to intermitting fever, in the treatment of which so much labour was often expended in vain, there was always the encouraging conviction that favourable results of all kinds were effected by it; old injuries of every possible character were removed, the gross humours were brought into freer flow, all damaged material was thrown off, and to a certain extent the whole body was rejuvenated. But perhaps many a panegyrist of fever might in truth have been inclined to agree with that author, who classes fever with catarrh, which is also held to be salutary, but who adds: "Ego tamen utràque carere malo." To this view of the curative effect of fever a certain amount of justification is not to be denied, when it is brought forward with the necessary restrictions. But latterly it has receded into the background. On the other hand, the conviction has been steadily gaining ground that, quite apart from the fundamental disease, fever of itself causes great danger to the patient; in fact, that in many febrile disorders the majority of the patients who die, die not of the disease proper, but of the accompanying fever. To this class belong, for instance, the various diseases which, on account of the severe affection of the psychical functions are called Typhus (τῦφος, =mist, smoke, confusion). In these and many other diseases the *indicatio causalis* and the *indicatio morbi* can be only imperfectly realised. In spite of all therapeutical efforts, the disease usually runs its natural course; but the patient escapes, if he be not killed by some especial complication, or by the accompanying fever more particularly. And in this fever consists the essential danger; and this knowledge has naturally led us to find in the fever the essential object of treatment.

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* Partly from a public inaugural lecture, delivered at Tübingen, 3rd Nov., 1871, "On the Recent Progress made in the Treatment of Febrile Diseases."

The dangers which fever brings with it will be at once clear, if we examine more minutely the pathognomonic symptom of fever, the rise in temperature, and take into consideration, on the one hand, the causes, on the other, the consequences of this increased heat.

Since it has been with certainty pointed out, that the immediate cause of the febrile rise in temperature is owing to an increased combustion, it is clear that in every fever an increased consumption of the corporeal substance takes place. And this is so much the worse, that at the same time the appetite and digestion are generally much affected, and consequently the replacement of the loss caused by the increased combustion is limited. The necessary consequence of every fever is, therefore, loss of substance, wasting, consumption, etc.

It is a decided step in advance that has of late years been made in the treatment of fever, that at present this febrile wasting is much more taken into consideration than formerly, and that we endeavour by dietetic and other rules to limit, on the one hand, the increased oxidation, and on the other, to promote the restoration of consumed material.

This danger of excessive loss is very prominent in chronic febrile complaints, those protracted, remittent, or intermittent fevers which we are in the habit of calling hectic. In fact, the wasting that occurs in Phthisis pulmonalis is frequently due, as a more exact analysis of the facts proves, in great part to the accompanying fever. The wasting may be absent, and there may even be, in spite of existing phthisis, a decided increase in substance, if there be no fever.

In acute febrile complaints, also, a rapid decrease of substance takes place. But this danger of wasting is not the greatest or most important. Countless human beings die of acute fevers before any noteworthy degree of wasting has been reached, and at the post-mortem examinations of those who have died of typhus, for instance, we often find the panniculus adiposus well developed, and the muscular tissue by no means atrophied.

But in severe acute fevers there is another and a more important danger, which was not unknown to the physicians of old, but which only in more recent times has been properly estimated and taken into consideration in treatment. By knowing and subduing this danger, the hitherto unheard of results have been chiefly attained, which later years can show in the treatment of acute fevers. This danger consists in the *rise of temperature itself*, in the injurious influence which such an abnormally high bodily temperature exercises on all the organs of the body.

If the bodily heat of a mammal is artificially increased five or six degrees, the consequence, according to the unanimous experience of numerous observers, is certain death. It is precisely the same with human beings. If a man's temperature rises to 42½°C. or higher, he is hopelessly lost. Certainly, the excessive rise in fever to more than 42° occurs but seldom, but the number of recorded observations in literature is already a tolerably large one; and all experience agrees that a temperature of 42° or 42½° is the limit, beyond which life can no longer exist. But if a rise in the temperature of somewhat more than five degrees is under all circumstances speedily followed by death, it may easily be supposed that the generally occurring increases of two, three, or four degrees, the temperatures of 39° to 41° cannot be quite harmless. And it is a fact that what happens quickly from rapid and excessive increase will just as surely take place, though slowly and gradually, with a lower but longer-continued increase of temperature. A man whose temperature persists at 40° or more, dies in consequence; one man perhaps dies after a few days, another after a longer period, according to the individual power of resistance.

The experience of the last few years, too, has thrown some light upon the manner in which a high temperature kills. In the case of a patient dying in consequence of a long continued and considerable rise in the temperature, we constantly find numerous organs in the body in a more or less advanced state of parenchymatous degeneration. Their actual cell-elements are degenerated, and often in great part completely destroyed. In the liver, for instance, we find the cells abnormally filled with granules; in many of them numerous fat-globules appear; the nuclei become invisible, at length even the sharp contours of the cell disappear, and the cell is represented only by a loose conglomeration of granular *débris*, and at last all cohesion ceases. Instances occur in which, in the whole liver, we cannot find a single cell in a state of perfect preservation. In these, the highest degrees of degeneration, it is anatomically essentially the same condition as in acute yellow atrophy. The small amount of blood contained in the minute vessels, and the proportionate alteration in colour of the cut surface, which only allows recognition of the lobular markings of the liver indistinctly or not at all, is generally sufficiently striking to the naked eye, even with a moderate degree of the degeneration; the colour becomes more greyish red, and in advanced degeneration it is often greyish yellow, or orange coloured; the consistency of the organ is lessened, though it often retains a certain amount of pasty tenacity. Similar alterations are found in the kidneys, chiefly in the epithelium of the cortical substance. In the heart are found indistinctness and obliteration of the transverse striæ of the primitive fibres; frequently they are filled with abundant fat-globules. The whole heart is generally strikingly soft, and is easily torn, the colour of the muscle is usually paler, more of a greyish yellow, often brownish. In the voluntary muscles we find the same indistinctness of the primitive fibres, caused by granular masses, consisting partly of fat, and often with it the so-called waxy degeneration, through which the muscular fibres are entirely destroyed. And at length similar alterations are found in the smaller blood-vessels, in the brain, &c.

We can consequently name a large number of organs, whose cell-elements degenerate and are at length destroyed, owing to the action of a long continued high fever-temperature.

These degenerations, which depend on fever, seem to have nothing specific about them; they are anatomically not essentially different from the changes which are effected by various other deleterious causes, such as the different poisons (phosphorus, etc.), or from those which appear occasionally to take place spontaneously and without any known cause. They seem to be one of the more common forms of "Necrobiosis," as they may be caused by the most varied influences, subject always to the condition of their leading to the gradual destruction of the tissues.

These febrile degenerations of organs occur most frequently in the severe contagious diseases that are accompanied by fever, apparently because these are mostly accompanied by very high and long continued fever; but they are also found in other diseases, in which contagion, in the ordinary sense of the word, is not to be thought of, and quite as constantly in all those individual cases in which the increased temperature has shown a corresponding height and duration; and, on the contrary, they are absent in those cases of typhus and other contagious diseases in which, owing to particular causes, the rise in temperature has no considerable height or duration.

But experience teaches us that the increase of temperature acts in a hurtful manner on the tissues otherwise than that already shown, so that all already existing disturbances of nutrition, or which may arise in the course of the disease, take on a more dangerous character and run a worse course. Surgeons have long known that healing wounds immediately become worse if the patient suffering from them be attacked by typhus, or some other severe febrile complaint. I have frequently observed, that after the commencement of an attack of abdominal typhus, a simple, soft chancre has become phagedænic and has led to extensive destruction; in one case amputation of the penis became necessary; in another case, with remission of the fever, a not very extensive gangrene became circumscribed, and healing commenced; but the patient relapsed, and with the return of the fever the whole scrotum became gangrenous, spreading from a bubo; on the cessation of the fever the part healed. In phthisis which has been long at a standstill, the destructive process is often set up anew by some complicating fever. And bed-sores generally first begin to cleanse themselves and to heal when the fever subsides.

The essential danger of fever in acute diseases consists then in "*the deleterious influence of a high temperature on the tissues.*" The material changes must necessarily lead to functional disturbances, and, in fact, these make their appearance in great variety. Among these functional disturbances two groups of symptoms are especially noteworthy, on the one hand, because they are the most striking, on the other, because the death of the patient is mostly caused through them. These are, the disturbances of the functions of the heart and of the brain, which finally lead to cardiac and cerebral paralysis.

The disturbance of the functions of the heart shows itself at first in an increase in the frequency of its contractions. That the increased frequency of the pulse, inasmuch as it is peculiar to fever, can only be looked upon as the consequence of the increased temperature of the body, is proved from the statistical comparison of a large number of observations, which shows that the frequency of the pulse is always greater in proportion to the height of the temperature. But it is easy to understand that this rule only becomes clearly defined when very numerous observations are compared; for in single cases many irregularities must be found, because most of those manifold individual and external influences which affect the pulse in a healthy subject may come into operation with a fever-patient.

The excessive action of the high temperature on the heart, which finally leads to cardiac paralysis, shows itself chiefly in an excessive rise in the frequency, together with great weakness of the pulse, soon to be accompanied by the other signs of general lessening of the circulation; among other things, hypostases take place, the peripheral parts become cold while the internal ones are hot; at length death results, mostly with symptoms of œdema of the lungs.

The disturbance of the function of the central nervous system, so far as it is peculiar to fever, must also be understood as arising from the effect of the high temperature on the central organs. This is proved by the impartial observation of the behaviour of the central organs in the exceedingly numerous cases of febrile disorders. However different the nature of these complaints may be, the disturbances agree completely in their principal features; and their nature and intensity, with the exception of special individualities, are only dependent on the height and duration of the rise of temperature. In pneumonia, facial erysipelas, and in acute articular rheumatism, the symptoms are the same, when the rise of temperature has the corresponding height and duration, as in those diseases commonly called typhus.

The knowledge of these febrile disturbances, a closer description of which would take us too far from our present subject, and the taking into consideration the fact of their dependence on the increased temperature, is of much importance in relation to diagnosis. It is a known fact, which frequently happens, that when a patient becomes delirious or falls into a state of stupor, the physician immediately feels bound to assume that there is meningitis, or some brain affection, or he thinks of uræmia or cholæmia, or some other toxæmia, while in reality there only exist the simple consequences of the rise of temperature. Of course, under certain circumstances it may be very difficult to decide if the existing disturbances depend on fever, or if a particular complication is to be put down as the cause; and in the most different febrile complaints, it is well known that such complications not uncommonly occur. Of the greatest importance for the correct determination of these points is the careful observation and the right judgment of the relation of the temperature of the body and its variations to these psychical disturbances.

In all cases, the influence of individuality is very great. There are men who become delirious with the lightest fever, while strong and long continued rise of temperature is required to cause delirium in others. With habitual drinkers, delirium more or less frequently takes the characteristic form of delirium tremens; and in very many, but by no means in all cases, the delirium potatorum is a simple fever-delirium in a peculiarly disposed individual.

It is further to be remarked that those psychical disturbances are by no means always the most severe that make the greatest impression on the public; that furious delirium, for instance, may, under certain circumstances, be less serious than a certain degree of stupor or coma. To judge of the degree of disturbance often demands a more than mere superficial observation, and the less striking, or, rather, more negative disturbances, simple loss of power or suspension of the functions, have often a far greater signification than those more striking qualitative alterations which express themselves in perverse action.

Finally, we have to consider that these psychical disturbances are owing to a material alteration in the central organs, caused by the rise in the temperature, and therefore that they cannot suddenly disappear on the cessation of that rise, but must necessarily last longer than the cause, though where the rise in temperature has lasted but a short time, as in the case of intermittent fever, the consequences of it are soon adjusted. And even with a longer continued fever there is usually, at the least, a marked diminution of the psychical disturbances after a strong remission. But if the increased temperature has lasted a very long time, the disturbances caused by it may, under certain circumstances, continue long after the temperature has again become normal. Not uncommonly, just at this time, they first take on their more striking forms. But the conditions of simple psychical weakness are especially those that last for such an extraordinarily long time, and only gradually disappear: it is, for instance, well known to be the rule, that after an attack of typhus that has been at all serious, many months pass over before the bodily strength is quite restored, or the mental capacities have recovered their former condition. Besides, fully developed mental diseases occur after severe fever, in the origin of which the previous increased temperature was not unconcerned.

The higher degrees of psychical disturbance pass at length, if the increased temperature still continue, into more or less complete arrest of the functions, and cerebral paralysis, extending to the spinal cord, causes death. But it is to be remarked that pure cases of this kind are rare, generally cardiac paralysis participates in the fatal termination. Cases of extreme prostration of the brain-functions are by no means desperate, as long as no appearances of cardiac paralysis are present, corresponding to other experiences which also show that temporary arrest of the function of the brain is better borne than an arrest of the function of the heart.

The more these views of the relation of the individual fever-symptoms to each other, and of the prominent signification of the increase of temperature become recognised, the more must be their influence on the theoretical maxims which regulate the treatment of fever. What some of the older physicians had, with a happy anticipation, conjectured, and more or less confirmed by direct experience, is proved theoretically to be the natural inference of this perception. If we know that in numerous febrile diseases the greatest danger is to be looked for in the long continued height of the temperature of the body—that the raising of the temperature is, in an extremely large number of cases, the direct or indirect cause of death—then is it intelligible that an essential problem in the treatment is to lower the temperature, to cool the patient. The numerous methods of treatment which we comprise collectively under the name of "*Antipyretic treatment*" answer this indication. Here belong, on the one hand, the direct cooling of the body, which might be separated from the antipyresis proper, and called antithermal or anticaustic treatment; on the other hand, the multifarious dietetic and medicinal prescriptions, which are intended to limit the production of heat, and which might be called antipyretic treatment in the narrower sense of the word. We shall, however, make no such distinctions in the following pages, but shall include among the antipyretic remedies all things that have the power to lower the temperature of the fever-patient. In fact, in the face of the great danger arising from the increased temperature, it does not matter particularly whether the excessive heat be symptomatically removed from the body as quickly as possible, or whether, to a certain extent, we attack the evil at the root, and endeavour to prevent the increased production of heat by removing the cause. The indication for cooling the body is so urgent that that method deserves, of all others, the preference which most surely succeeds in lowering the temperature. And, in order to oppose the deleterious action of the increased temperature, such means even are justifiable which may be not only unpleasant to the patient, but may even be followed by some slight injury.

In what manner is this indication to be accomplished? How can we manage to lower the temperature of a fever-patient?

The answer to this question appears very easy at first sight. If you want to cool a piece of hot iron, it is only necessary to plunge it into cold water. If, therefore, we recognise the essential danger of fever to be the excessive height of the temperature of the human body, it is evident that the cold water treatment is the simplest and most judicious method.

If the affair were really so simple as some physicians represent, we may be certain that Hippocrates would have treated all his fever-patients with cold water. For both he and most of the physicians of antiquity, as is undoubtedly shown in their writings, had a very clear perception that in many cases the excessive heat was the cause of mischief; and individual attempts to conquer this fever-heat with cold water have been reported from those times. More frequent attempts, however, were first made since the commencement of the last century, when some physicians treated their fever-patients more or less methodically with cold water. They had, however, but few followers. In the latter respect greater success was achieved by an English physician named James Currie, who, in the last decade of the last century, employed the cold water treatment, chiefly in the form of cold affusion in various febrile diseases, principally exanthematic typhus and scarlatina, and who also endeavoured, by his experiments on healthy subjects, to establish the mode of action of cold water. Currie is, with justice, universally looked upon as the founder of modern Hydropathy. His method found abundant imitation, and most of the reports as to the results appear favourable. For a time it would seem as if the cold water treatment would gain the day, and would be generally recognised and made use of. And during the first years of the present century a prize was offered in Germany for the best work on the "External Application of Cold Water in Hot Fevers," and three papers were sent in.

But the stimulus given by Currie was not effectual for any length of time. His method gradually fell out of use, and at last was almost forgotten. Even Priessnitz and his followers, who cured everything with cold water, used it least of all against fever-heat. Only a few individual physicians still used cold water to treat fever; but either they limited their observations to a few cases only, or they had not the courage to carry out the method with that energy which alone has power to ensure brilliant results. Almost the only one who employed the cold water treatment with the necessary consistency was Ernst Brand, in Stettin. His book, published in 1861, about the hydropathic treatment of typhus, although in many respects one-sided, and often, instead of objective representation and criticism of facts, merely showing the enthusiasm of the subjective conviction, was, in spite of this, well suited to once more draw attention to the method. It has done good service in stimulating other physicians to make trial of this method, and this stimulation we have to thank, among others, for the energetic and experimental researches of the Kiel observers.

But how was it possible that a plan of treatment which, as the experience of the last few years has shown, has furnished such extraordinarily favourable results, should have become entirely unknown, after it had once got into tolerably extensive use? In truth, a remarkable and instructive historical fact!

Many reasons have been brought forward in explanation. The method did not agree with the theory of some physicians; others perhaps were unreasonably afraid of public opinion; while others again might well be shocked at the incomprehensible demeanour of the so-called Hydropaths, with whom an educated physican would not willingly be classed. But all this is insufficient to explain the fact; such obstacles would certainly have been easily overcome, if it had really been so easy to convince oneself of the excellence of the method. There was, however, one other still greater obstacle which stood in the way of the general introduction of the method; and this must be kept in view and put on one side if we would wish to avoid a repetition of the same occurrence. People had a false idea as to the mode of operation of the abstraction of heat from the human body; they expected more from it than it could really accomplish; in fact, *the method did not answer their expectations.*

The living human body, with which we have to deal, is a wonderfully complicated organism, which behaves itself in very many respects differently to a lifeless mass, and which reacts to many external influences in a quite peculiar and often unexpected manner. If we wish to change this organism in any definite manner, we must know its peculiarities, and we must know how it responds to the various agents. The theory of the action of the individual remedies must be laid down up to a certain point; for it is only by knowing the difficulties that we can hope to overcome them. It was the want of theoretical knowledge of the operations of the cold water treatment which prevented its success. And if we would encourage the further propagation of this method, and at the same time avoid its falling into disuse, after being again heard of for a time, we should endeavour to obtain accurate researches as to the mode of action of the abstraction of heat, together with the further communication of trustworthy therapeutical and statistical materials. Only when the illusion is dispelled, that the whole thing is so very simple, as many are disposed to regard it, and when the operations of the withdrawal of heat, with respect, at least, to its coarser conditions, are sufficiently known, are we assured of the knowledge of a means of cure in future, which is in the position of few others to save the endangered life.

In healthy men, an ordinary cold bath does not lower the temperature in the interior of the body; frequently, indeed, a slight increase takes place during the bath. But after the bath, when a pleasant feeling of warmth sets in, the temperature sinks somewhat in the interior of the body. During the bath, the body protects itself against the cooling process, on the one hand, by weakening, as much as possible, the operation of the abstraction of heat, by limiting the circulation on the surface of the body; and on the other, by replacing the abstracted heat by an extraordinary increase in its production of heat. But if the withdrawal of heat be too great, we can succeed in cooling a healthy man even during the bath; if, for instance, he be plunged into ice-water, or if the bath continue too long; in that case the regulating power is to a certain extent overpowered. The boundary in regard to the intensity and duration of the abstraction of heat, at which the regulating force is sufficient to maintain the temperature tolerably constant, is different in different individuals. For instance, a man having his panniculus adiposus strongly developed, being thereby more protected against the withdrawal of heat, does not require to produce so much heat as a thin man in a bath of equal temperature and duration; and the former too can far longer offer opposition to the cooling of his interior. All this takes place in the same manner with a fever-patient; there is, however, this difference, that with him the means do not extend so far, and that, consequently, the limit at which the refrigeration of the interior begins is somewhat sooner overstepped. But the fever-patient has just as much a tendency to persist in keeping up his temperature; he protects his abnormally high temperature against cooling in the same manner as the sound man protects his normal temperature. But at the same time the already high production of heat in a fever-patient is increased in an extraordinary degree by a cold bath; as is proved by direct determination of the heat-production, and confirmed by the determination of the quantity of carbonic acid.

It is this persistence of the regulation of heat in a fever-patient which presents the essential obstacle to a satisfactory lowering of the temperature of the body. This explains why it is that many observers, who have commenced the cold water treatment with the greatest hopes, but have only used it to the extent of slight withdrawal of heat, have been disappointed in their expectations.

There is no doubt, that through the withdrawal of heat, at least during its continuance, the consumption of the constituent parts of the body is increased; the increased production of heat and of carbonic acid during the bath is a proof of this. This is a circumstance that must certainly be kept in view, for under certain conditions it may actually contra-indicate the practice of energetic heat-abstraction. But this increased consumption can hardly be taken into account, when we consider the danger which arises from the rise in temperature alone in by far the greater number of cases of acute febrile diseases. But besides this we find, as continued observation shows, that the increased consumption that takes place during the withdrawal of heat is compensated by the fact that for some time afterwards the production of heat descends below the usual degree.

Is, however, the immediate object, the cooling of the body, really attained by the withdrawal of heat? And, if it be attained, how does this agree with the fact, that during the fever the regulation of heat continues?

In a fever-patient we frequently observe that the temperature in the interior does not sink, but rather rises, as the first and direct effect of a withdrawal of heat. On the one hand, however, we should notice, that this only occurs in abstractions of moderate intensity; and, on the other hand, that the temperature sinks on the cessation of the withdrawal of heat; and the reaction shows, as in a healthy subject, a more decided sinking than the previous rise of temperature. Accordingly the desired effect, which is frequently absent during the withdrawal of heat, is yet always attained after the cessation of it.

But whilst in withdrawals of heat of lesser intensity, the favourable operation depends exclusively upon this after-effect, in the withdrawals of greater intensity another and more important circumstance comes into consideration. In healthy men we have power to reduce the bodily temperature, in spite of the most perfect regulation of the heat-production, by overcoming this regulating force by very intense or long-continued abstractions of heat. With fever-patients the same thing is possible; and a lesser intensity and duration of abstraction suffice to overpower this regulating force than would attain this purpose in a healthy man.

It follows from the practice already cited, that in fever every external cooling is immediately followed by an increased production of heat, but that, in spite of this, we are in a position to reduce the temperature of the fever-patient by withdrawals of heat. This succeeds, on the one hand, owing to the after-effect, on the other, by the forcible overpowering of the regulating force. In spite of all obstacles, and in spite of the effective resistance offered by the organism, the object aimed at is attained by abstractions of heat of sufficient intensity, either by the cold bath or the cold douche; the body of the patient is thoroughly cooled both externally and internally. A tolerably strong effort is, however, necessary in order to reduce the temperature one or two degrees. There still remains another difficulty; and this perhaps is principally the cause that has so long prevented the introduction of an effectual cold water treatment. When a patient in the heat of fever has had cold water dashed over him, he has felt himself apparently better, directly after this violent treatment. But the fever-heat was, as the old observers used to expect, in no way permanently lessened, or at least not for any length of time; generally after a few hours the condition was the same as before. And if one could have made up one's mind to repeat this highly disagreeable proceeding daily, it would have had no perceptible influence on the course of the disease. And until within the last few years the few physicians who were still in the habit of using the cooling treatment have nearly all made the same mistake (I must reckon myself among them, for I have fallen completely into the same error), that it was a sufficiently hazardous matter if a fever-patient was subjected every day to such violent treatment; only occasionally have we dared to give a bath twice; and these baths were, besides, generally not as cold as they should have been.

The great merit of having cleared the way in this direction belongs to Bartels and Jürgensen, of Kiel. These observers have shown, that in the majority of patients thoroughly cold baths may be used without danger as often as may be necessary, that is, as often as the temperature in the interior of the body may again have risen beyond a certain limit. The great results of the cold water treatment date from the publication of these experiences in the year 1866. It was throughout shown that such great results could only be attained by controlling the temperature of the patient by day and night, and using the cold bath as often as the bodily temperature required it. It is usually necessary to give four or six, or eight cold baths in the 24 hours. In severe cases, it happens that the bath has to be given twelve times in the 24 hours. I have treated some typhus-patients where the total number of baths used exceeded two hundred. These were extremely obstinate cases, in which death would certainly have resulted in consequence of the fever had a less energetic treatment been employed.

The cold water treatment has hitherto been used most frequently in cases of abdominal typhus; and where this treatment has been properly followed up, the results have been surprisingly favourable.

In the Kiel Hospital, from 1850-61, out of 330 typhus patients, 51 died under ordinary treatment, that is, 15·4 per cent. On the other hand, from 1863-66, out of 160 cases which were consistently treated with cold water, only 5 died, that is, 3·1 per cent. And later still, the results of this treatment appear even more favourable.

The antipyretic treatment has also afforded surprising results in the hospital at Basle, where abdominal typhus is extremely common, and very virulent. I will give a statistical comparison of the mortality, with the various methods of treatment.

Until the year 1865 the treatment was of the ordinary symptomatic and expectant kind; though in the later years they had begun to give an occasional cold or tepid bath. When in August, 1865, I undertook the direction of the department, baths were given more regularly, but still generally only once a day, rarely twice. At the same time, in order to aid the antipyresis, quinine or digitalis was used, but not in so energetic a manner, nor according to such established indications as later. At length, however, from September, 1866, after having seen the convincing communications of Jürgensen, of the results obtained in Kiel, baths were employed in gradually increasing frequency, and by degrees somewhat colder, until at last, since the commencement of the year 1868 the method of treatment has been fairly settled.

I. *With ordinary treatment.*

Year.	Typhus Patients.	Died.	Mortality.
1843 to 1853	444	135	30·4 per cent.
1854 to 1859	643	172	26·7 „
1860 to 1864	631	162	25·7 „

II. ***With imperfect antipyretic treatment.***

Beginning of 1865 to September 1866	982	159	16·2 „

III. ***With consistent antipyretic treatment.***

September 1866 to the end of 1867	339	33	9·7 „
1868	181	11	6·1 „
1869	186	10	5·4 „
1870	139	10	7·2 „
	845	64	7·6 „

The numbers which form the basis of these statistics are not directly comparable, because formerly the expression "typhus" was used in a rather more restricted sense than at present, so that many of the slighter cases were excluded from it. But, as has been fully shown in another place, the limits of any error arising therefrom can be determined, and in so doing it is proved to have but little weight. After the most careful examination of all circumstances that may be considered, we find that in the last few years the antipyretic treatment gives hardly a third of the fatal cases which would have occurred had an equal number of similar cases been treated in the ordinary manner.

Reports of the effects of the antipyretic treatment in abdominal typhus come from numerous other hospitals, and almost without exception the results are remarkably favourable—namely, in Munich, Erlangen, Würzburg, Nuremberg, Greifswald, Jena, Halle, etc. In Vienna alone the new method does not appear to answer properly. At the seat of war and in the military hospitals, numbers of physicians have approved of the antipyretic treatment, and even in private practice it is being extensively carried out with success. This mode of treatment is not alone useful in abdominal typhus, but, generally it is of value, especially in every other disease accompanied by fever, in which the rise of temperature of the body brings danger by its height or duration; and the region of these diseases is now proved to be much greater than had formerly been anticipated; all complaints belong directly to it which, as a rule, are accompanied by somewhat severe and continued fever. The most brilliant results are naturally to be expected in those diseases in which the danger chiefly or exclusively depends on the fever, but only slightly on the local disturbances, as, for instance, in abdominal typhus, exanthematic typhus, scarlet fever, etc. But much is gained even in those diseases in which severe and dangerous local lesions exist, if we can succeed in allaying the danger arising from the fever. For instance very favourable results are furnished by the consistent use of the cold water treatment in those severe forms of pneumonia which we usually call typhoid, bilious, or asthenic; the mortality is thereby considerably diminished, though not to the same extent as in typhus, as I can testify from my own experience, having treated more than 200 cases of pneumonia in this manner.

But besides this, I have hitherto employed cold baths in scarlatina, measles, in the primary fever of small-pox, in pleurisy, in puerperal fever, and in meningitis cerebro-spinalis epidemica, of course only in such cases in which the fever seemed to threaten danger by its intensity or duration.

The temperature of the baths, as I am in the habit of using them in hospital and private practice, is generally 20°C. (=16°R.), or less. For one patient the same water is mostly used for several consecutive baths; the bath stands, in the meantime, in the patient's room, and by the time of the succeeding bath the water gets to about the temperature of the room, and then it is not further altered. Only in hot summer weather, or when, from particular circumstances, a lower temperature is required, it may be restored by putting in pieces of ice. The length of the bath amounts, as a rule, to ten minutes. A much longer time is mostly very disagreeable for the patients, and possibly might be productive of injurious consequences. If a weak patient be so affected by the bath that he continues shivering for a long time afterwards, or even collapses, it is advisable to limit the duration to five or seven minutes. Such a short cold bath is always more effectual than a far longer continued tepid bath. Directly after the bath the patient must rest; for that purpose, without being first dried, he must be wrapped up in a dry blanket, laid in bed, which may be somewhat warmed at its foot-end, lightly covered up, and, under certain circumstances, should have a glass of wine given him, and only after some time has elapsed should his night-dress be put on. With very weak patients we can commence with baths of a higher temperature (24° C.=20° R.); for such cases, when external considerations permit, the gradually cooled baths of Ziemssen are useful, where one begins with about 35° C. (=28° R.), and by slowly adding cold water brings the temperature gradually down to about 22° C. (=18° R.); such a bath must have a longer duration.

Cold douches, as direct calometric examination shows, have much less effect than cold baths of equal temperature and duration, and they are much more unpleasant to the patient. They could therefore only be recommended, if external considerations rendered impossible every other kind of effective heat-abstraction, or if one intended less a cooling effect than a strong stimulation of the psychical functions or of the respiration.

Cold packings are generally well borne, even by the weakest patients, particularly if the feet and legs are left free. A series of three or four consecutive packings of from ten to twenty minutes each, have about the same effect in cooling the body as a cold bath of ten minutes' duration. Particularly with children, in whom, on account of the relatively large bodily surface, all heat-abstractions act much more strongly, and with whom, therefore, baths are used either less cold or for a shorter time, they can be completely replaced by cold packings. Cold sponging, even with ice-water, appears to have but a slight cooling action; this, by frequent repetition, might perhaps be increased so as to be of some value. It cannot, however, take the place of the baths.

Local abstractions of heat by cold applications, ice-bladders, etc., appear to have no essential influence on the temperature of distant parts of the body. A general antipyretic effect might perhaps be obtained if as large a surface as possible of the patient were extended on large pillows filled with some cold mixture (Leube).

In a healthy man the bodily temperature becomes reduced by cold drinks, swallowing ice, and by cold enemata, etc., and about as much as corresponds to the quantity of warmth required for warming the introduced material: a regulated increase in the production of heat does not take place, as in the cooling of the external skin. This is a circumstance of essential signification: and if the general effect of such abstractions be not very great, we have this advantage that there is no opposition to be overcome, and that no great demand is made on the system. If it were possible to abstract large quantities of heat from the internal organs without causing much irritation, this would be theoretically the most commendable form of refrigeration. From this point of view, a proposition which was made to me by a colleague may appear worth a trial, namely, to wash out the intestinal mucous membrane for a length of time with a constant stream of cold water, by means of a double-action œsophageal-tube carried far up the rectum, one pipe of which should be put in communication with the reservoir.

The extreme obstinacy of fever in many cases, which is occasionally not to be overcome by the most persistent use of baths, as well as the circumstance that many patients will not bear a sufficiently frequent employment of them, renders it necessary to use, besides, other remedies which may contribute to the attainment of our purpose, the reduction of the temperature of the body. Among other suitable medicines, we may mention as the principal, quinine, digitalis, and veratrum.

Quinine has, setting aside the malarious fevers, a decided antipyretic action only when given in large doses; to a grown person I give, as a rule, 1½ to 2½ grammes of the sulphate or muriate of quinine (I find no difference in the action of these salts), but it is essential that *the whole of the dose be given in the course of half-an-hour, or at the outside, in the course of a whole hour.* If the dose were to be distributed over a length of time the action would be perceptibly less; even a much larger dose, but given in the course of half or a whole day, has often scarcely any marked influence on the temperature. But, on the other hand, it is unnecessary, as a rule, to repeat this dose before the expiration of 48 hours. I should also like to call attention to the fact that quinine, contrary to a very general opinion, is less indicated in those cases in which the fever has strong spontaneous remissions or intermissions, than in febris continua or subcontinua. Its favourable action depends on its causing an intermission of the fever, even though only a temporary one. Where such intermissions occur spontaneously, just this indication no longer exists.

The known fact, that an extremely violent fever, which, however, has occasional complete intermissions, is much less dangerous than a less violent fever which is continuous or only shows slight remissions, as well as the experience that this rule is also valid for the artificially produced abatements of the fever, has induced me to direct my efforts to the attainment of the most perfect intermissions possible in the employment of antipyretic remedies. I consider, therefore, the action of a dose of quinine as only then sufficiently complete, when by it the temperature is reduced to its normal height, that is, at least below 38°. Should this occasionally not succeed with the first dose, a larger dose is given the next time. But if, and this is not of uncommon occurrence, the temperature sinks at the first dose below 37°, the next dose may be made somewhat smaller. This is the simplest way to individualise and to suit the quantity of the dose to the individual case. I have in this way employed quinine in abdominal typhus, and have also used it in pneumonia, in the simple, as well as in the typhoid or asthenic forms, also in variola, scarlatina, erysipelas, acute articular rheumatism, pleuritis, in the symptomatic fever that accompanies suppuration, in phthisis florida accompanied by severe continued fever, and in meningitis cerebro-spinalis epidemica. An antipyretic effect is never absent, if the case be sufficiently carefully watched, and if a large enough dose be given in a short space of time; yet there appear to be certain diseases in which it is more difficult to obtain the full effect of the quinine—I may casually mention acute articular rheumatism and the secondary fever of variola. But in all other febrile diseases also, it not uncommonly happens in particularly severe and obstinate cases, that even the dose of 2½ grammes is inadequate to render the temperature completely normal. In such cases the end can often be attained by other antipyretic remedies, or by a judicious combination of them with quinine.

In employing digitalis for the sake of the antipyresis, I use it only in substance in the form of powders or pills. This form is far more reliable when its antipyretic action is required, while the more generally used infusion deserves perhaps the preference where we chiefly intend to affect the action of the heart. When given in substance, it is well known that the full dose is considerably less than when used in the form of the far less active infusion. I give generally ¾ to 1½ gramme, distributed over about 36 hours. Digitalis in severe febrile diseases in general is so much the less indicated, the more excessive is the frequency of the pulse; it seems as if, under certain circumstances, it had the power of hastening the appearance of impending cardiac paralysis. On the other hand it can be given in abdominal typhus with good results, as long as the action of the heart is not as yet immoderately frequent, or at least has still some power in it. In particularly severe and obstinate cases, if a satisfactory reduction of the temperature is not obtained by means of quinine, the desired effect may mostly be attained by a combination of digitalis and quinine. A dose of from ¾ to 1½ gramme of digitalis in substance may be given gradually in the course of 24 or 36 hours, and directly upon that a full dose of quinine (2—2½ gr.). If once we are successful in obtaining a complete intermission by this means, quinine alone generally succeeds later.

Veratrum is a very reliable antipyretic, if used in sufficient doses; it often effects a complete intermission in cases where it had not been obtained by quinine. I generally give pills, each containing 5 milligrammes, one every hour, until decided nausea or vomiting ensues. Generally four to six pills suffice. The collapse, which readily follows the vomiting, with the rapid decline of the temperature, is not dangerous even in typhus patients, and is usually quickly removed by wine or other analeptica.

I have not hitherto had sufficient experience to be able to state anything positive about the action of other strong antipyretic remedies.

In the cold water treatment, as well as in the use of the other remedies mentioned in the large doses ordered, we have to do with a highly energetic, and, one may say, violent proceeding, which must be carried out with great consistency, if we expect to obtain favourable results, but which, like every other energetic kind of treatment, must be variously modified to suit the peculiarities of individual cases, and for which, consequently, the careful direction and supervision of an experienced physician is a necessary condition. But the experience of thousands of years has shown that in this region of little expedients no great results are to be expected, that rather, as in everything, great effects presuppose great causes. And, finally, the surgeon's knife is no plaything which can be trusted to an officious or inexperienced hand.

The extraordinary practical results which have already been produced by antipyresis carried out in a systematic manner, prove, more than all theoretical deductions, that we are in the right road. Enlarged experience and the further perfecting of the theory will contribute to secure the way, and we also hope to make it more even and convenient.

In a measure, as an example, I will give a short sketch of the *antipyretic treatment of abdominal typhus*, as it has been gradually established in the course of time in the hospital at Basle. If the patient comes for admission before the ninth day of the disease, we give him at first two to four doses of calomel of ½ gramme for a dose, in the course of a few hours. If there should chance to be any severe diarrhœa present, it would be at the worst only momentarily increased, on the whole rather diminished. In by far the greater number of cases a considerable remission of temperature is thereupon observed. Day and night, from the time of admission, the temperature is taken in the axilla every two or three hours, and as often as the thermometer reaches or exceeds 39° C., a bath of 20° C., or less, and of ten minutes' duration, is given. Cases of any degree of severity, where six to twelve baths are given in the 24 hours, receive on the second evening 1½ or 2 grammes of quinine; the determinations of temperature, and the baths as often as they are necessary, not being interrupted in the meantime. If towards morning the temperature falls below 38°, and continues there so long that for the next 12 hours or more no bath is required, then 48 hours after the first dose a similar or perhaps even a smaller one is repeated; but if the fall in the temperature was not sufficient, a larger quantity would be given for the second dose. Should this prove sufficient, the same or a smaller dose is repeated every two days, as long as the continuation of the fever appears to render it necessary. In those very severe cases, in which even the dose of 2½ grammes does not prove sufficient, we pass on the morning following the last dose of quinine to digitalis, and in the course of the next 36 hours give gradually from ¾ to 1½ grammes of digitalis in substance, the pulse and temperature being in the meantime under constant control. Directly afterwards, that is, 48 hours after the last dose of quinine, a similar one is repeated. Then the temperature almost always sinks below 38° by the following morning, occasionally down to 36°, and then it often happens that the obstinacy of the fever is so far broken during the remainder of the illness, that later the baths and the doses of quinine repeated every two days are sufficient; perhaps eventually the combination of digitalis and quinine might have to be once more repeated. But, should it happen, which is very seldom the case, that even digitalis and quinine obtain no satisfactory remission, there still remains the use of veratrum, which occasionally suffices to lessen satisfactorily the obstinacy of the fever in its further course. Where, on account, perhaps, of intestinal hæmorrhage, or from some other circumstances, the use of the baths has to be interrupted, quinine can still generally be given from time to time. If intestinal hæmorrhage has occurred, it is usually given in solution with the addition of tinct. opii. Under other circumstances, again, it might be given as an enema.

In the severe asthenic, typhoid, or bilious forms of pneumonia, which frequently occur in Basle, a similar plan of treatment has generally been carried out, that is, cold baths and quinine used according to similar indications; only in those cases with very high temperature (about 41° or more) a full dose of quinine is mostly given soon after admission, and with it digitalis in substance. Quinine has, too, the advantage over digitalis as an antipyretic in the treatment of pneumonia, that a remission of the fever is sooner obtained.

A similar proceeding answers equally well in other severe febrile disorders; but we must carefully individualise, according to the circumstances of isolated cases, and also according to the nature of the disease and the expected course of the fever. For instance, in an ordinary intermittent attack, or in a simple febrile angina, even if the highest temperature should be perhaps reached, it would hardly be necessary to set the whole antipyretic apparatus in motion; rather would we hope that without this the increased temperature would have no long duration, and therefore would cause no particular danger. And just in the same manner with a typhus patient; if, in the later stages, satisfactory morning remissions or even intermissions should occur spontaneously, if even in the evening the temperature should again rise, we should not easily be induced to act antipyretically with great energy.

I make no use of the stronger heat-abstractions in chronic febrile affections, such as pulmonary phthisis. The increased production of heat, as well as the waste of material which takes place during the heat-abstractions, seems to contraindicate such a proceeding in a disease, one essential danger of which consists in its wasting. But I willingly admit that direct experience can alone decide if it be justifiable, as possibly here a complete or even incomplete compensation might occur as after-effect.

Quinine in large doses is the more indicated in phthisis, the more the accompanying fever, as in many cases of phthisis florida, approaches the type of a febris continua. We occasionally succeed, by giving one or more large doses of quinine at intervals of two days, in not only temporarily lessening the fever, but even in considerably weakening it for a length of time. But we must, at the same time, take into consideration that in many such patients there exists a highly fatal circle, inasmuch as the local affection sustains the fever, and the progress of the local disease is favoured by the height of the temperature. If we are successful in breaking through this circle, it may, under some circumstances, have a permanently good effect. The constant use of digitalis and quinine in smaller doses is advisable in cases of phthisis with decided remitting or intermitting fever. We give generally about 0·1 digitalis and 0·3 to 0·5 quinine in pills, and continue them till the effect is obtained, or until the disturbing consequences of the action of the digitalis set in, as, for instance, loss of appetite, nausea, or even vomiting.

As this paper has become rather lengthened, I will not enter upon the rules of diet, which are of such importance in the treatment of febrile diseases; besides, there is no difference of opinion among physicians in regard to the more important points; for instance, as to the necessity for perfect rest, both bodily and mental, the care for suitable position and cleanliness, and as to the importance of pure and not too hot air, and of plenty of cool drinks.

One short remark might not be out of place here respecting the choice of nourishment for a fever-patient. With regard to wasting febrile diseases, I especially wish simply to recall to mind the universally known experience concerning the so-called Banting-diet, which has furnished sufficient explanation of the fact that beefsteaks and eggs are not the kinds of nourishment to restore power to a phthisical subject, but which rather shows that the mode of nourishment which experience has pronounced to be judicious, that in which the hydro-carbons, and especially the fats (cod liver oil), are amply represented, is theoretically justified for such cases; and as in acute febrile diseases, fats in large quantity are not digested (not absorbed), there remain as proper kinds of nourishment only those that contain chiefly hydro-carbons, and we thus arrive at that fever-diet which experienced physicians since the time of Hippocrates have been accustomed to use. Modern time has made one essential advance in adding the knowledge that alcoholic drinks do not increase fever, but rather have the power of diminishing it.

ON PELVIC MEASUREMENT.

BY

PROF. R. DOHRN.

G. A. Michaelis has laid down the proposition that the narrow pelvis is more frequently disadvantageous owing to consequences proceeding indirectly from it, than it is through the direct difficulties which it gives rise to at the birth. How well founded this assertion is every one will admit who has had the opportunity of observing a number of labours accompanied by narrowness of the pelvis. The cases in which the skull of the child carries away an impression of the pelvic wall are rare compared to those in which the narrow pelvis gives rise to anomalies in the posture or position of the child.

This circumstance more than any other has hindered the practical acquisition of a knowledge with regard to the narrow pelvis. The more remote consequences which the narrow pelvis gives rise to frequently by themselves alone form the real difficulty in the labour. In cases of this kind there is the danger that we may overlook the cause whilst attending to the effect. When it is a case of cross-birth we have no trouble in finding abundant cause for the difficulty of the labour; but to discover whether or not it is the narrowness of the pelvis which causes the cross-birth requires a deeper, special attention on our part.

The frequency of the narrow pelvis is variously estimated. This really results from the difference in the care which is bestowed upon the diagnosis. If we only diagnose a narrow pelvis because the labour is hindered by a bony projection, we shall find but few narrow pelves; but if we consider that the measurement of the pelvis is an indispensable part of an obstetrical examination we shall rate their frequency higher. It is worth nothing for a man to tell us that in some lying-in hospitals narrow pelves seldom occur, and at the same time to tell us that pelvic measurements are not regularly made there. The only person who has a right to make an assertion with regard to the frequency of narrow pelves is he who can say that he is accustomed to measure the pelvis regularly in every case of midwifery that comes before him.

Where a sufficient number of cases have been examined in the latter manner, the results vary but little; consequently it follows that the difference of race, and the diversity in the mode of life of the population within the German boundary, are not of sufficient importance to cause any marked variations in the shape of the pelvis. If we consider that a pelvis is narrow when its conjugata vera measures 9·4 ctm., or less than that, then, according to Michaelis, who examined a thousand cases in the hospital at Kiel, the frequency of the narrow pelvis is 13·1 per cent. Litzmann, also in a thousand cases, found 14·9 per cent.; Schwartz, amongst 501 cases at the hospital at Marburg, found 20·3 per cent., and amongst 463 cases at the hospital at Götting he found 22 per cent.; Spiegelberg, amongst 597 labours at the lying-in hospital at Breslau, found 13·9 per cent.; and Schröder, amongst 321 cases in the hospital at Bonn, found 14·6 per cent.

These numbers are of sufficient importance to claim the attention of the practitioner. The only grounds on which his neglect of them could be excused would be that he could show that the narrow pelvis had no effect upon the course of the labour, or, at least, that our treatment had no effect upon the evils proceeding from it. But it is exactly those cases of labour which admit of the most successful treatment that are most influenced by the narrowness of the pelvis. For instance, Michaelis has shown that, in a collection comprehending 849 cases of labour, cross-births were four or five times, face presentations eight times, and head and funis presentations ten times as frequent where the pelvis was narrow as they were where it was large: however, anomalies in the position and posture of the fœtus often permit correction during pregnancy.

If then the experience of our ablest observers leads them to the conclusion that the diagnosis of the pelvis demands the closest attention, it can only be right that the physician should admit pelvic measurement as a necessary part of an obstetric examination. Who indeed would consider that the investigation of a chest disease was complete where the physical examination had been omitted? And the same holds good with regard to the measurement of the pelvis in midwifery cases.

In many instances indeed the history of the case draws our attention to the investigation of the pelvis. Where facts are stated which point to an earlier existing rachitis or osteomalacia, or where we hear that the previous labours have been difficult, we ought to examine the pelvis with special care. Though of course in these cases both positive and negative statements must be received with caution; thus for example it is by no means certain that rachitis will leave a deformity of the pelvis after it, and on the other hand this disease may exist, even to a high degree, without our being informed of the same by our pregnant patient. Here, as well as elsewhere, an examination decides the point.

We will suppose the patient placed on her back, and that we begin with the examination of the *large* pelvis. Anyone fairly expert may by a manual examination arrive at results which are of value diagnostically. With nothing but the hand we can very well determine whether the iliac bones are properly expanded, and whether the hips are well- or ill-developed. The determination of the size of the different parts and their distance from one another is on the contrary more difficult. Even the most expert observer is apt to make considerable errors if he employ manual aid alone. We shall find that the help of a pair of caliper-compasses is absolutely necessary for making an exact examination.

This instrument, which was first introduced into midwifery practice by Baudelocque, has in the course of time received many improvements which have very much facilitated its employment by the practical physician. If a simple instrument is wanted, E. Martin's calipers will be found useful when the adjusting screw is removed. If on the other hand we wish to arrive at the greatest possible accuracy, we must use Lambrecht's excellent calipers, such as are employed in the hospital at Götting. There should be no adjusting screw on the 'calipers, for their points always have to be adjusted by the hands of the physician. If there is a screw, and one wishes to make the calipers fast with it and has no assistance, there is nothing else to be done but to remove the hand from the points, and then the measurement will be inaccurate; also the removal of the fixed instrument is often accompanied by pain.

With regard to the question as to what diameters are worth our attention in the exterior measurement of the pelvis, there are two things to be borne in mind. Firstly, we must take those measurements only which are characterised by well-marked points; and secondly, those only which give us at least an approximate idea of the *internal* diameters which we are seeking. If we keep to these rules we shall not recommend for measurement any diameter whose extremities are buried beneath a thick layer of cutaneous tissue or which possesses no sharply marked bony points. Michaelis measured four external diameters—(*a*) the distance between the anterior superior iliac spines; (*b*) the distance between the iliac crests; (*c*) the external conjugate diameter; (*d*) the distance between the trochanters. But it did not escape the notice of such an attentive observer as Michaelis that the trochanteric measurement did not possess the same value as the other three. The bony surface is too large, the covered soft parts are too thick, the length and direction of the neck of the femur too variable for anyone, except in rare cases, to make use of this measurement with advantage for the determination of the width of the transverse diameter of the pelvic cavity.

One usually finds that the iliac spines stand forth as well-marked bony points. It is but rarely that one finds them so little marked that there can be any doubt as to which point ought to be selected for measurement; but when this is the case it is advisable to let the points of the compass glide together several times over the spines from their outer to their inner border in order to find the right spot. The outer border (where the tendon of the sartorius muscle takes origin) should be chosen as the measuring point. The distance between these points in well-built German women is found to be 26·3 ctm. These figures are the result of an average of the measurements of 500 pregnant women whose pelves Michaelis measured. The different results at which some German authors arrive as to the *normal* distance between the iliac spines can be explained partly by differences in the mode of measurement and partly by cases of narrow pelves not having been sufficiently excluded. C. Martin's published estimate of 25 ctm. originates in the latter manner; a statement which, owing to its introduction into the Prussian medical calendar, has found a wide-spread distribution amongst practical physicians.

We must now pass the points of the calipers, under the constant direction of the fingers, from the spines backwards along the outer lip of the crest of the ilium. We must then observe at what point in this measurement the calipers are widest apart, and reckon the distance between their points as the width of the crista ilii. In normal pelves one finds that this is on an average 29·3 ctm. If the arc of the iliac ridge is altogether wanting, as is sometimes the case with rickety pelves, then the measurement may be taken 6 ctm. behind the spines, according to Michaelis' proposition; but practically it is quite sufficient to know that the distance between the crests does not exceed that between the spines.

For the measurement of the external conjugate diameter the hips should be somewhat raised, or, what some consider as more convenient, the back position should be changed into the side position. With the right hand one then seeks the pit below the spine of the last lumbar vertebra and sets one end of the calipers there, holding it with the hand, the other branch is then placed on the centre of the upper border of the symphysis pubis; the normal measurement here amounts to 20 ctm.

In the greater number of cases one may content oneself with these three measurements as far as regards the exterior of the pelvis. Accuracy of measurement, sufficient for the satisfactory verification of the foregoing numbers is easily gained, even by practitioners who have not before attempted these investigations. Exactitude is most easily attained in the measurement of the distance between the iliac crests. With some little care in the repeated measurement of the same person one can satisfy oneself that this measurement is correct to within a few millimeters. The measurement of the spines is somewhat more difficult; and most difficult of all is that of the external conjugate diameter, in which the soft parts exert more influence than in any other measurement. In the latter case the measurement varies according as the cutaneous coverings are more or less compressed; by practice and patience, however, quite sufficient practical accuracy can always be attained.

With regard to the employment of these external measurements in practice, we may consider it as a primary maxim that they give us more certain information respecting the *shape* of the pelvis under investigation than they do respecting its *capacity*. For instance we are very well able to say from a comparison of the three measurements whether the pelvis which we are examining belongs to the group of the so frequently occurring flat pelves, or whether it preserves its normal shape; but we can on the contrary only approximately draw any conclusion as to the *amount* of narrowing which has resulted from this flattening. If we find the transverse diameters large and the difference between them but slight, we may conclude that we have to do with a flat pelvis. If we find a normal proportion between the three external measurements, we may conclude that the brim of the pelvis has a normal shape.

With regard to the estimation of the size of the small pelvis from the external measurements, we must look upon the transverse diameter of the pelvic inlet as the internal measurement corresponding to the external transverse diameters. We may conclude that the internal diameter is small when we find the external transverse measurements considerably shortened, and we may infer that it is large when the external measurements exceed the normal. In the latter case, however, we must bear in mind that the transverse diameter of the pelvic inlet does not increase in the same proportion as the *external* transverse diameter; usually it only increases to a much less extent, for with the increase of the external measurement a greater development of bone generally takes place. The external conjugate diameter corresponds to the conjugata vera. But the ratio of these two measurements one to the other is as variable as that of the foregoing, and one must never found any important obstetrical treatment upon the idea of a fixed relation between them. In this case the diagnosis would be much influenced by the internal measurement of the pelvis. Only so far can we go in the use of the measurement of the conjugata externa, viz. that where this differs several centimeters from the normal, we may infer that there is a greater or less conjugata vera, as the case may be. The strength of the bony structures is evidently also worth consideration in this estimate.

Besides the three foregoing measurements various others have been recommended for employment in external pelvimetry, and have obtained an entrance here and there into the practice of the physician. The most important of these is the external oblique measurement, in which the distance from the posterior superior iliac spine to the anterior superior iliac spine of the opposite side is measured. The value of this measurement has been thoroughly tested of late, and the favourable expectations which were attached to it by some have not been verified. The external oblique measurements do not comply with the above-mentioned conditions as they ought to do when an external pelvic measurement is considered worth recommendation. For the posterior superior iliac spine is not a good measuring point, as it does not offer a marked bony projection to the touch. According as one applies the points of the calipers under or over this, so the measurement varies. But if it is allowed that by great practice the inexactitude resulting from this fact can be much lessened, there still remains another difficulty which diminishes the value of this measurement, viz. that the ratio between the external oblique measurement and the internal one corresponding to it at the inlet of the pelvis which we wish to discover, is so variable. How great this variation is has been shown by Schneider and Gruner. We may draw such a wrong conclusion in this way, that from an external oblique measurement we may diagnose a deformity of the pelvis exactly contrary to that which really exists. Though the external oblique measurement may be of value in some cases, yet it must be admitted that more recent investigations always show that the practical use of this measurement is very precarious; and in the present state of opinion with regard to pelvic measurement we can with our utmost endeavours only get those few measurements adopted in practice which have been proved useful in the greater number of cases.

Even the measurement of the circumference of the pelvis is taken by some, and usually in this manner,—a measuring tape is carried from the spine of the last lumbar vertebra outwards between the iliac ridge and the trochanter major and then forwards to the symphysis pubis. These measurements give us no practically useful information. The internal measurement which corresponds to the external pelvic circumference is the periphery of the pelvic inlet. But any conclusion which is drawn from the *size* of the periphery of the inlet is pretty nearly worthless. For a pelvic brim the periphery of which measures 45 ctm., may have such a form that the labour proceeds without difficulty, whilst another which has precisely the same periphery may have such a shape that the child's head passes with the utmost difficulty. But in addition the periphery of the pelvic inlet cannot by any means be estimated from the external pelvic circumference. At the spot where the measuring tape is applied the soft parts are very fully developed, the skin has a thick under layer of fat and the muscles are very large. According as these are more or less developed, so does the amount of the pelvic circumference vary. In fat persons we get a large, and in thin persons a small measurement.

Just as little is to be gained from a measurement of the pitch of the pelvis which is occasionally taken in cases of labour. To ascertain this exactly is much more difficult than is usually supposed. In the large pelvis we have no mark whatever to enable us to determine the position of the plane of the pelvic inlet. We can form but an approximate idea as to the pelvic inclination from the position of the vertebral column and the shape of the pelvic region, and we are further taught by the researches of Herm. Meyer, that the pitch of the pelvic inlet varies from time to time in the same person, and to a much greater extent than was formerly supposed. If we recollect, as Michaelis has pointed out, that a defective pitch of the pelvis is usually compensated for by a corresponding position of the uterus, we shall be much more inclined to recommend that the practitioner turn his attention to other inquiries which give more exact results and are more useful. The same holds good for the numerous other exterior pelvic measurements, which have been from time to time recommended without their utility having been thoroughly proved.

It consequently comes to this, that we need take but *three external* measurements in order to make a precise examination of the pelvis. Anyone who will accustom himself to measure his patients in the manner I have proposed will soon learn in what cases he may content himself with exterior measurements and in what cases others are necessary. The more exactly one measures and the more carefully the three averages which have been given are used, the less often will one make use of any other external measurements.

When by external examination we have learnt as much as possible with regard to the form and capacity of the pelvis in question, we may then proceed to the *internal* measurement to complete our investigation. The patient should lie on her back, and it is indispensably necessary that the hips be well raised, so that the arm may have free play for the introduction of the hand. We must also take care that the patient is brought close to the edge of the bed during the examination. The more convenient her position the more exact and careful will be the measurement. The "cross-bed" (Querbett) is to be preferred before all other means of support in difficult cases.

A great number of instruments have been contrived for the internal measurement of the pelvis. Not one of these has proved to be of use practically; every new attempt at instrumental measurement has on the contrary served to show how much superior the hand is to the new instruments as a means of measurement of the internal cavity of the pelvis. It is true we are very limited in our manual measurements when we endeavour to obtain numerical results. The conjugata diagonalis is the only diameter which we can correctly measure manually; the others we can only estimate; in spite of this limitation, however, we can with care diagnose the capacity of the pelvis with quite sufficient accuracy for practice. Endeavours have been more especially made to measure the conjugata vera instrumentally. But all have been futile, and opinion with regard to their result is so universal that a criticism of the various means resorted to would be superfluous. We can only indirectly estimate the conjugate diameter of the brim from the diagonal diameter, and with what certainty this can be accomplished in some cases Michaelis' example may teach us, who, though he must have got his practical dexterity from little material, yet reached an exactitude in the calculation of the conjugate of the brim, which may even to this day be taken as a model. His directions for the measurement of the diagonal diameter have proved practically superior to any others for the last twenty years.

I prefer the index and middle finger of the left hand for the measurement of the diagonal diameter; let them be well oiled up to the metacarpus and then introduced upwards along the posterior wall of the vagina. If we are examining a patient in the last month of pregnancy we shall seldom find any difficulty in the performance of the measurement. If however there is any difficulty, we shall do better by not endeavouring to overcome the resistance of the perinæum and other soft parts by forcible stretching; relaxation of the parts will occur much sooner if we carefully employ a continued upward pressure. When we have reached the posterior vaginal arch we must press this upwards and backwards carefully, and whilst moving the finger which we have introduced up and down gently, we must seek the nearest bony point opposite the symphysis pubis. We must press the tip of the middle finger against this spot and then raise the wrist in such a manner that the lower border of the symphysis comes into contact with the fold of skin between the thumb and forefinger. When this is accomplished, we must with the other hitherto unemployed hand mark the point of contact between the symphysis and the before-mentioned fold of skin. Michaelis performs this manipulation in the following manner: the right hand is brought into extreme pronation and the nail of the forefinger is pressed in whilst the tactile surface of the latter turns upwards against the symphysis. The plan which others adopt is more simple, for they mark with the tactile surface turned downwards; but we must be careful that we do not catch hold of the soft parts of the introitus or the pubic hairs, and that the mark is not placed too far forwards.

When we have thus marked the length of the diagonal conjugate we draw the fingers out of the vagina, taking great care to keep them in the same position as they were in the pelvis; we then measure the distance with the calipers.

We may attain to very considerable accuracy in this measurement. In ten cases which Michaelis was able to measure after death he found that he had at the utmost only made an error of a line and a half. Though a knowledge of the size of the diagonal conjugate is in itself most important, yet the chief use we make of it is to estimate from it the size of the conjugata vera.

The ratio between these two diameters is subject to much less fluctuation than that between the external conjugate and the conjugata vera. For from the conjugata externa we can only approximately estimate the size of the conjugata vera, but from the conjugata diagonalis we can do so very accurately.

The normal measurement of the conjugata diagonalis is 12·6 ctm., the normal measurement of the conjugata vera 10·8 ctm.; therefore the difference is 1·8 ctm. The fluctuations in this difference depend upon the depth of the pubic symphysis and its inclination towards the conjugata vera. If the symphysis is deep and but little inclined, we must make a great deduction in order to calculate the vera from the diagonalis; if the reverse is the case only a slight deduction must be made. In consequence of this the exact observer will not be content with an average estimate, but will decide for himself what deduction should be made in each case separately. Observation has taught us that the pitch of the symphysis is much more important than its depth. We must therefore endeavour to get at the utmost possible accuracy with regard to the former. No one has attained so far as to measure the exact angle which the conjugata vera makes with the diagonalis on the living body; but we can without difficulty manage to find out whether the pitch of the symphysis is considerable, moderate, or little, and then we must make a deduction in proportion. With regard to the depth of the symphysis, this factor permits of exact calculation because we can feel its borders externally. Thus of the two factors which regulate the difference between these two conjugata we are able to determine one with accuracy and both with at least approximate exactness; and in this way a sufficiently precise calculation of the conjugata vera is possible. If another point of the posterior pelvic wall than the anatomical promontory lies opposite and nearest to the pubic symphysis, then we must measure the conjugata diagonalis to this point; and if this false promontory is high up we shall have to make a great deduction in the calculation of the conjugata vera, but if we find it deeply placed within the cavity of the pelvis we shall have to make a small one.

With regard to the second diameter of the pelvic inlet which is of practical importance—the transverse—we cannot get such satisfactory results by an examination. We cannot directly measure the transverse diameter of the pelvic inlet, nor can we calculate it from the other diameters with safety. We can only approximately estimate it from, firstly, the size of the external transverse diameter; and, secondly, from the amount of difficulty which occurs in feeling the lateral walls of the pelvis from the interior. If the latter investigation is to be of any real service, we must be able to eliminate the deceptive influence of the soft parts within the cavity of the pelvis, and this can only be done by very great practice. Our estimate of the size of the pelvic inlet in a transverse direction will therefore never be other than inaccurate, and in some cases it will only be an unusual position of the head which will draw our attention to the fact that the pelvic inlet has been narrowed in its transverse diameter. And with regard to this point it need only be observed that a strikingly deep position of the small fontanelle, where it cannot be explained by the position of the uterus or peculiarities in body of the fœtus, always makes it extremely probable that there is a transverse narrowing of the pelvis, for when the latter is the case, the forehead, which is but little exposed to the pressure of the uterus, is liable to be detained on the pelvic brim.

An exact measurement of the pelvic space below the brim is not feasible; in the living body, marked bony points which can be taken as starting-points in the measurement are entirely wanting. In *extreme* degrees of narrowing we can sometimes make use of a two-legged measuring instrument in order to examine the roominess of the pelvic cavity in various directions; but in moderate degrees of narrowing such an instrument can no longer be used, for the separation of the legs stretches the vagina too much. And owing to the want of fixed points of measurement it is difficult and in many cases impossible to apply the legs in one uniform manner; therefore such instrumental measurements cannot be depended on.

The foregoing holds good also with regard to the pelvic outlet. Narrowings here are often enough diagnosed in practice; and yet our science has not hitherto advanced beyond the dictum of Michaelis, who stated in his work upon the narrow pelvis that he could never make sure by measurement that there was narrowness in the outlet. We can much more easily form an opinion as to the roominess of this part of the pelvis by feeling round the interior in various directions with the hand. If we proceed with caution a noteworthy narrowing of the small pelvis in the one or the other diameter will not easily escape us.

On the other hand anyone who thinks he has got a positive numerical result by measurement on the living body would do well not to neglect to test the same by repeated measurement; often enough the result of an earlier examination will be modified by a later.

Therefore the internal as well as the external *exact* measurements that we can make are very few. But when we have taken into consideration the various circumstances affecting the pelvis, when the bony protuberances have been examined, when the three external pelvic measurements have been taken, when an endeavour has been made to measure the conjugata diagonalis, and when by an internal examination we have felt round the pelvic walls in various directions with the exploring finger, we shall, in the majority of cases, be able to make a very satisfactory diagnosis of the pelvic proportions. That this much is done ought to be expected from every exact and observant accoucheur. The pelvis is certainly only one factor which comes into consideration with regard to the labour, but it is one which is exceeded in importance by no other; and if we do our utmost in every case that comes before us we shall soon find to what a certainty we have arrived in our treatment.

PRINCIPLES OF THE

TREATMENT OF CROUPOUS PNEUMONIA.*

BY

PROF. JÜRGENSEN,

KIEL.

GENTLEMEN,—I open for you to-day a page from the daily life of medical practice. It is an old, much-discussed page, but still one to which decade after decade adds its store of knowledge and ability.

How shall the physician treat, at the bedside, a patient attacked with pneumonia?—not, how shall the physician treat pneumonia? As I have chosen to put the question in this way, I may as well indicate my position at the outset.

Every one who has seen a large number of cases of pneumonia, knows that however much they may correspond in the principal phenomena, yet they present differences which are in no other disease so multifarious; and these differences even affect those principal phenomena. "Croupous pneumonia" is rather an anatomical diagnosis based on distinct physical signs, than a pathological grouping of even approximately constant symptoms. Call to mind, in the first place, the pneumonia of old persons, and then remember the picture presented by a patient affected with this disease in the bloom of life. Can there be a more glaring contrast in the phenomena? And yet the dissecting knife very often shows in both the same changes.

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* From discourses in the Poliklinik. The fundamental ideas of this lecture were laid before the Medical Association of Schleswig-Holstein and the Naturforschersversammlung, in Rostock, as early as 1871.

[A few alterations and additions have been made from MS. notes kindly furnished by the author for this translation.]

It is therefore necessary that we should assert with special emphasis that, particularly in pneumonia, the subject of treatment is not the disease but the patient. Theoretically, of course, the correctness of this statement is self-evident; but if you observe actual practice, or what is written in text-books, you will agree with me on the necessity of placing this matter, apparently of A B C, in the fore-front.

You know how ready I am to separate clinically the smaller related groups from the traditional picture of "acute," "genuine," "primary" pneumonia. From the standpoint which we occupy to-day, however, that, namely, of therapeutists, we must not only give up these distinctions, but include in the subject of our discourse even cases of secondary pneumonia, in so far as they are croupous.

"Croupous pneumonia," as it is defined anatomically, and not how or where it arises, or how it appears clinically, is the subject of consideration here.

Much that will be said applies to the other forms, but not to all.

What variations from the normal does pneumonia always present? We ought to look for a distinct answer to this simple question. Yet we must remember that we treat here of roughly defined groups, and that we must reserve to another occasion the researches which lie at the basis of our knowledge of the actual facts.

Recognising this restriction without reserve, we answer—

Pneumonia produces

1. *An interference with the function of the lungs.*
2. *Fever.*

Everything else which may be observed is to be referred to these variations from the normal, or else it is not necessarily connected with the essential phenomena of pneumonia. The disease never runs its course absolutely without fever. The height of the fever, however, shows the greatest variations. In emaciated old persons the temperature (which, if not the fever itself, is a true criterion for it) often rises only a little over 38° (100°·4 F.); cases are observed where it reaches 42° (107°·6 F.).

That the function of the lungs is interfered with we learn from dissection. The extent and the character of the exudation show at least as much variation as the fever.

In the *second* place, we inquire as to the special danger to which the life of a pneumonic patient is exposed.

OF WHAT DO PNEUMONIC PATIENTS DIE?

Is the affection of the lung by itself, or the fever by itself, sufficient to warrant one in saying, in the concrete case, that death is inevitable from the one or the other? The question must be answered distinctly in the negative.

Consider the condition of a person 20 years of age with an infiltration of the whole left lower lobe, and a temperature of 40° (104° F.) on the sixth day of the disease, and then look at the same person a few hours later, after the natural crisis has occurred, and his temperature has sunk to 37° (98°·6 F.). Will anyone assert, with the shadow of probability, that there is any essential alteration in the state of the lungs? Just as little will anyone, on the occurrence of a fatal œdema of the lung in the last moment before the crisis, lay the whole blame on the lungs as such, and on them alone. Consider further that, as a matter of experience, we find that a chronic pneumonia with resulting cirrhosis, or a pleurisy with compression of the lung and retraction of the thorax, produces a much more serious disturbance of the function of the lung than pneumonia, and yet may be for years well borne. On the other hand, compare the condition in pneumonia with that of an equally serious case of typhus at a similar period of the disease. If the fever alone is sufficient to explain the occurrence of death in pneumonia, then we ought to find an equal number of persons dying of both diseases in the same period of time, if the temperatures be equally high. According to the results obtained by Liebermeister's pupils, the percentages of deaths in the first week, as compared with the total deaths in the two diseases, were for typhus 31·8 per cent., for pneumonia 75·5 per cent. These results may have only an approximate worth in respect to the absolute figures; but the proportion between the two is no doubt generally correct. From this alone we can infer that the mortality of pneumonia is not to be deduced from the intensity of the fever. There are other points of view from which this opinion is also fully confirmed. You will permit me, in order not to tire you by repetition, to leave these considerations aside.

In order to avoid the accusation of dogmatism, I will acknowledge that there are isolated cases of pneumonia in which death occurs just at the beginning, before there can be much interference with the lungs, where the fatal result may be due to the intensity of the fever alone. This rare occurrence has however no influence on our principal statement; it is of quite subordinate importance.

But if, with this exception, neither the fever nor the lung affection is sufficient of itself to prove a source of danger to the pneumonic patient, what then? Are we to look for a third hypothetical consideration, or endeavour to come to an understanding with the considerations already before us? I think the latter.

Neither of these two factors is sufficient of itself to endanger the life of the patient; but the disturbance of the function of the lungs and the fever are both injurious. Nothing is therefore more natural than to suppose that acting together they may produce what they were unable to do separately. Two possibilities present themselves:

The lung-affection and the fever may have each of them a distinct point of attack peculiar to itself, or both may have a common one.

I hold the latter to be the correct view, and consider it capable of proof. I lay down the following statement therefore:

The danger in which a croupous pneumonia places the life of the patient threatens, in the first place, THE HEART. THE FATAL CASES OF PNEUMONIA DIE FROM INSUFFICIENCY OF THE HEART.

The foundations for this statement are these:

The pneumonic exudation increases the resistance in the pulmonary circulation, and thereby necessitates an increase of labour on the part of the right ventricle.

The exudation in the lung exercises pressure on the surrounding structures. According to the law of the impenetrability of space, the exudation requires room for itself. The forces by which it is moved are sufficient to push aside the original contents of the part.* These contents are: the air which fills the bronchi and alveoli, and the blood which occupies the vessels in the diseased portion of lung. The displacement of these is only possible to a certain extent, because the pressure of the exudation is opposed by other forces, chiefly those which move the air and blood in the lungs. The motive force of the exudation on the one hand, and the muscular force exercised by the action of the heart and respiration on the other, go into the lists against each other. The amount of the displacement therefore depends on the varying effects of these three factors.* But one thing is certain: that as soon as the state of equilibrium is reached, then *the amount of blood to be pressed through the affected part is weighted with greater obstructions.* The position of the vessels of the lung which are situated *outside* the inflamed part, is similar to that of a system of tubes in which a part of the exit-pipes has been closed, but through which it is still necessary to pass a quantity of fluid as great as when the system remained open. This can only happen by increase of the propelling force. This means in our case, that in order to prevent the circulation through the lungs from being retarded by the infiltration of the pneumonic portion, there must be more force supplied by the right side of the heart, which drives the blood through the lungs. The left ventricle has also to bear the burden of the nutrition (bronchial arteries) in the inflamed portion of lung impeded by the pressure of the exudation, however insignificant this burden may be.

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* For the completion of the argument, the increase of volume shown by the diseased part in every case of pneumonia, may be mentioned here. It

2. The changes produced in and on the lungs by the pneumonia, cause a diminution in the total amount of force for the movement of the blood furnished by the lungs themselves.

The lungs do, by means of the muscles of respiration, assist in the circulation of the blood. The lungs themselves are perfectly passive. But in order that a portion of the vital force furnished by the muscles of respiration may be used in carrying

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is well known that in extensive infiltrations, there is not infrequently a mould of the ribs on the surface of the inflamed portion of lung.

* As a matter of fact, the conditions must be very complicated *during* the exudation, because a very considerable part of the exudation is derived from the blood-vessels, and therefore has the same motive power as the blood. The coagulation of the exudation brings this to a close—and the description given above refers to this period of time.

on the circulation, the lungs must be capable of changing their volume—of being filled out and again drawn together. If now a portion of lung is in a state of pneumonic condensation, it is no longer in a position to alter its volume. But the amount of force exercised on this portion remains the same as before. The amount of force exercised by the motive power—the muscles of respiration—must remain the same, because it acts on the thorax as a whole. And since the vital force furnished by these muscles is only available through the extensibility of the lungs, then so much force is lost for the circulation, as under normal conditions is assigned to the infiltrated part.

The loss of force thus produced by the altered physical conditions, is made still more considerable by a physiological condition which is seldom absent.

In the great majority of cases, the inflammation of the lung is accompanied by inflammatory processes on the corresponding portion of pleura. In this case pain is produced whenever the shifting of the folds of the pleura during respiration produces irritation. There is consequently always an effort to make this shifting of the pleural surfaces as limited as possible; that is, to make the respirations shallow. But opposed to this there is the more urgent necessity that the respirations should be sufficient. Under these conditions, all that can be attained is effected by, as it were, weighting the diseased side, which even with this cannot be kept quite at rest. This theoretical problem is in nature solved instinctively. In most cases the patient lies on the affected side. By this means the muscles which move this half of the thorax have to raise the additional weight of a portion of the body. The force thus expended only acts partially on this side of the chest which is so weighted; and in consequence it is less expanded. The other methods used by pneumonic patients who suffer from the pain—such as tight bandaging, holding the part tight, curvature of the vertebræ so that the ribs approach—have all the same effect, only differing quantitatively. As on the weighted side, the whole lung, and not merely the diseased part, is less expanded, there is here also a loss of a certain portion of the vital force furnished by the respiration in aid of the heart.

It is impossible to decide, *à priori*, to what extent the increased exertions of the respiratory muscles are capable of bringing up to the normal the amount of force supplied for carrying on the circulation. The more active the muscles are, however, the greater are their claims on the force of the heart. For the muscle which is acting more strongly requires more blood; in the contracted muscle the vessels are compressed, and therefore during the contraction there is an increase of the resistance to the circulation. This increased resistance affects the left ventricle, and is therefore of minor consequence.

3. The diminution of surface by which the blood and air come in contact in the lungs, produced by the pneumonic exudation, leads to an increased demand of force to move the blood and air, in order that sufficient aëration of the blood may take place.

When the exudation fills both bronchi and alveoli there can be no interchange of gases. The sum of the effect of each respiration—whether deep or shallow—is therefore necessarily reduced by the amount which, under normal conditions, the affected part would furnish. And if in a given time the aëration is to be as efficient as possible, then the forces by which air and blood are brought to the place of interchange, must increase. The muscular tissue of the heart, as well as the muscles which carry on the respiration, must produce an excess of force. As already mentioned, the increased activity of the respiratory muscles indirectly affects the heart.

4. The fever brings into prominence the local disturbances produced by the pneumonia.

This fact needs no proof. Every pneumonia which has reached the crisis, and which shows in a few hours a diminution of temperature to the extent of 2°—3° C., teaches us that the dyspnœa and increased activity of the heart are only conditioned by the fever. The lung disease has not in this short period retrograded sufficiently to induce us to suppose that the great change which we observe is due to a diminution in its intensity. But if anyone is still unconvinced by this argument, there is still another which is incontrovertible. The same diminution in the frequency of the pulse and respiration, the same improvement in the condition occurs, when the temperature is brought down to the normal by a cold bath, and the improvement disappears as soon as the effect of the bath passes off and the temperature again rises. No one will assert that here the local disturbances have disappeared to be again reinstated.

The only question which remains, is, in what way this actual effect is produced by the fever?

This question is not difficult to answer. The answer is summed up in the next proposition.

5. The fever brings about an increase of the heart's action, while it directly injures the heart.

We can also prove this proposition.

The most constant symptom of fever is increase of temperature. With increase of temperature there is an increase of the frequency of the pulse (Liebermeister); that is, the number of the systoles increases in a given time, and it follows from this that for that given time the total duration of the systoles is increased, while that of the diastoles must diminish.* In other words, the period of activity of the muscular tissue of the heart is increased, its period of rest diminished; there is consequently an increase of force required of it as compared with the normal.

In fever the formation of carbonic acid is increased (Liebermeister, Leyden). If then it is not to be accumulated in the organism, it must be more quickly eliminated than normally. This can only occur by an increase of the force furnished by the heart and muscles of respiration, as has already been fully described.

The fever leads to a degeneration of the muscular tissue of the heart (Zenker, Liebermeister). As the amount of vital force produced by muscle in its contraction is proportionate to its mass—that is to say, the number of the primitive fibres of which it is composed—then any diminution of the number of these must be equal to a diminution of the active power of the muscle. The same applies to each individual primitive fibre. Its active power again depends on the amount of contractile substance contained in it. And now it is determined anatomically that the fever leads to both a diminution of the number of the primitive fibres (from destruction of a certain number) and a diminution of the contractile substance of those that remain (from accumulation of fat and detritus). In consequence of this the fever leads to a diminution of the active power of the heart as a whole. The same conclusion is reached, when we consider that the material which results from the transformation of the contractile substance, acts by reason of its weight and elasticity as an internal resistance, in order to overcome which a certain portion of the vital force evolved during the contraction of the heart is absorbed. The contraction would be impossible unless this resistance were overcome.

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* The supposition, which might seem possible theoretically, that the duration of the individual systoles diminishes with the increase of their number, does not certainly apply in any appreciable degree in the majority of cases. It is only necessary to observe the pulse in order to be convinced of this.

We have also to consider the indirect injury which the fever exercises on all the organs of the body, and among the others, on the heart and respiratory muscles. The diminution in the amount of food taken, which, in highly febrile states, almost amounts to complete starvation, and exists to some extent in all cases, will affect most powerfully those parts which are most exercised. These are, again, the muscles which are continually in action—the heart and respiratory muscles.

You see how the lines converge from all sides to the one central point, how it is the heart, always the heart, which in the last instance is weighted. And it is a simple conclusion from these premisses, when I summarise the duty of the physician at the bedside of the pneumonic patient, as follows:—

The physician requires to put the heart in such a position during an attack of pneumonia, as will enable it to accomplish the increased labour entailed upon it by the disease. This general statement of our duty has two subdivisions.

1. Prophylaxis against weakness of the heart.
2. Treatment of the weakness of the heart, which has already occurred.

Is it possible, by removing the local changes in the lungs, to remedy the disturbance of function of the lungs? Many are of opinion that it is possible, by energetic treatment at the outset, to diminish, or even cut short, the inflammation of the lungs. But what reasons can be given for this statement? We meet with abundance of suppositions, based only on the authority of the writers, or the dogmas of others—there is hardly any disease which has been so much the pampered darling of the school at the time predominant. But it is as useless to demand reasons for this dogma as for any other. On careful investigation it turns out that all we can detect behind the "many a time," "sometimes," "often," with which these recommendations are introduced, is either a subjective intuition, or a tradition followed without being subjected to proof. And how is it possible to furnish any sufficient reasons for such a statement? It is not to be forgotten, that conditions which have an exact clinical resemblance to the commencement of a pneumonia, may entirely disappear in the course of 24 to 36 hours, and that the duration of many cases of fully developed pneumonia is only a few days. I will here simply indicate one other cardinal question. Is the anatomical alteration in the lung, which occurs in pneumonia, the dominant fact in the disease, or is it something secondary, only attaining to significance afterwards? In other words, is pneumonia a local disease, in which the inflammation of the lungs—however it may have arisen—conditions all the other symptoms? or do the changes in the lung and the other symptoms, especially the fever, depend on a fundamental cause common to both? In my opinion the latter is the case, and I will return to this point on another occasion. The treatment of the local changes, therefore, has only significance from the point of view which I previously indicated, and stands in a similar position to the treatment of the intestinal affection in typhoid fever. It matters little, however, whether anyone agrees or disagrees with this opinion, for it is quite certain that there is no agent which acts on the local disease of the lung, or at least we know of none.

The fever therefore remains the object of attack for therapeutics. And that this is the case is most distinctly indicated by the fact which we have already urged more than once, that though the local changes may remain, the assault of the disease is broken as soon as the fever disappears.

In the present state of our knowledge, the prophylaxis against weakness of the heart, in pneumonia, coincides in essentials with the treatment of the fever. Certain circumstances are to be specially considered here.

I have on previous occasions laid down this axiom, and I believe it to be unassailable in the treatment of typhoid fever, *sine thermometro nulla therapia.* That is to say, in this disease the temperature is of itself of such predominating importance, that it is only exceptionally (in the case of hæmorrhage from the bowel, perforation, &c.), that we need to consider anything besides. The temperature, as measured by the thermometer, is therefore the absolutely safe guide in the treatment of the patient. This is not the case in pneumonia. Here the axiom stands *sine pulsu nulla therapia.* And if you ask me whether I would rather treat a severe case of pneumonia without the thermometer or without the observation of the pulse, I would have no hesitation in saying, away with the thermometer. It is necessary to state this very distinctly, and in this case to return to the standpoint of our forefathers, however eager the modern spirit is for exact measurement. This is all the more necessary, as the thermometer is such a simple instrument that we can explain its use for therapeutic purposes to the stupidest nurse. The estimation, or even the observation of the pulse, requires peculiar dexterity, which is only attained by long experience. The sphygmograph is hardly an instrument for the bedside outside the clinique—all honour to it when properly used. If the pulse shows in any way (and there is much to be considered besides its frequency) that it is all right with the heart, then we can set the temperature aside as not, for the moment, of absolute importance, even though it should be over 40° (104° F.). On the other hand too low a temperature, if in the concrete case it involves the heart too much, calls for interference.

We have next to consider the method which is to be preferred in treating the fever.

Is it allowable to put a pneumonic patient into a bath, in order to withdraw heat?

It is not to be denied that reasons suggest themselves *a priori,* which demand the most serious consideration exactly from the standpoint which I have already taken up. We may be sure that every bath, from the moment at which the superficial vessels contract from the cold, produces an increased resistance in the vessels, and an increase of labour for the heart. It is a question whether in this way a complete paralysis of the over-weighted heart may not be produced. The increased production of heat occurring in the bath itself will act in the same direction, and so will the increased demand on the heart and respiratory muscles due to the increased elimination of carbonic acid.

These are truly very important considerations. We can only meet them in an essential way by facts of experience. And I can distinctly state that such an occurrence has never happened to me or my pupils, although, so far as I know, no one has used the bath so frequently, so cold, or so continuously in the individual case. I do not in the least doubt that during the bath, collapse leading on to death has occurred, nor that the bath has directly led to this result. But this collapse can be prevented if we observe a very simple rule. A temporary increase of the labour imposed on the heart is to be expected. Is it not possible to place the heart temporarily in a position to satisfy this increased requirement, to increase its production of force? Certainly, that is always possible. Only, no one ought to select a patient who lies in the last extremity, as the object of a bath experiment, and I would emphasize the term "last." For if it is the last but one, it is often possible to keep the patient in life. I shall hereafter indicate what requires to be done in such cases.

Another objection to the cold bath—and one which interferes with this mode of treatment in general, both among medical men and others—is this, it is said that the patients *catch cold.* The dread lest this should occur makes intelligent men as frightened as children who are told about the black man. And indeed the black man and catching cold are in fully developed pneumonia (I acknowledge the influence of catching cold as a cause which may lead to pneumonia) objects of equal importance. In order to get rid of this spectre, which for centuries has restlessly wandered about, I will here bring forward the results of my own experience, views which are founded on abundant proof. The pneumonic patient needs above everything fresh air. If it is possible to furnish this without giving rise to a draught, it is well. If, however, I have to choose between bad air and a draught, then I select the draught, and my patients do not suffer. I even allow them to use the bath under these circumstances, though after coming out of the bath their wet skin must be exposed to a current of air. How frequently do the circumstances of my patients at the poliklinik (out-door dispensary) force me to avail myself of such primitive methods as a broken pane, or a raised window, for ventilation. And yet I seldom see the orthodox "complications from exposure to cold"; bronchial catarrh, pleurisy, pericarditis, etc. Nor have I observed any effect on the production of sequelæ, which develope from the local affection—chronic pneumonia, phthisis, etc. I should not hesitate, therefore, in cases where abstraction of heat was called for, and no water was to be had, to expose my patient to the cold air till the necessary amount of cooling was reached. This proceeding would produce a much greater degree of subjective discomfort than the water bath, but would all the same advance the cure of the patient. My experience therefore justifies me in asserting that in pneumonia direct abstraction of heat is allowable, in spite of the well-founded theoretical objection to this procedure, and in spite of the groundless prejudice against it. I am fortunate in being able to adduce here the weighty authority of Liebermeister. He carried out, in his hospital at Basle, the purely antipyretic treatment of pneumonia, and he found that the mortality diminished from 26·1 per cent. to 16·5 per cent. That this was due to the therapeutic interference will be acknowledged by every one who reads the thorough work of Dr. Fischer, one of the pupils of Liebermeister, which has appeared in the 11th volume of the "Deutsches Archiv für Klinische Medizin."

We have to consider to what extent the abstraction of heat should be resorted to. Here again I am guided by this axiom, *the abstraction of heat is intended as a prophylactic measure for the heart, and the fever is the heart's most dangerous enemy.*

In strong persons, who are moderately ill, and without existing complication, we may adopt the rules which are generally followed in the cold water treatment of typhoid. A bath of the ordinary temperature of spring water is given as often as the rectum temperature reaches 40° (104° Fahr.). The duration of the bath is determined by the degree of its action

It varies between seven and twenty-five minutes. This is a simple formula, which includes the peculiarities of the individual, such as size of body, amount of fat, or the special circumstances of the disease, such as the intensity of the fever, as well as the external circumstances, such as the temperature of the water. But this rule does not apply to every case of pneumonia; cases are not infrequent—as in old people, or those who are very stout, or in weakly persons—in which the temperature only occasionally or never reaches 40°, but rather ranges between 38°·5 (101°·4 F.), and 39°·5 (103°·2 F.). These cases require peculiar care, and I take advantage of the diurnal rise and fall of the temperature, which is never absent in any febrile disease, and runs parallel to the normal variations. Lukewarm baths (20—24 R., 77—86° F.) in the early morning (4—7 a.m.), lasting twenty to thirty minutes, are sufficient in these cases to reduce the temperature for a relatively long period of time. A simultaneous dose of quinine confirms the action of the bath. During the continuance of the low temperature, the heart may, as it were, take its rest. A repetition of the bath at another part of the day, is, of course, not excluded. If anyone makes it a rule to observe the pulse carefully, he will soon find out the proper directions for his interference.

In young children we may avail ourselves of moist sheets. But we must not be satisfied with surrounding the chest alone with cold cloths. The abstraction of heat effected by these means is very slight, and, at the most, is only sufficient for infants. The thermometer must be diligently used as a guide in these cases. I am not at all fond of these packings, which cause much more disturbance to patients and attendants than the baths, and are much less certainly effective.

On the other hand there are cases of pneumonia, in which the intensity of the fever, measured not only by the absolute height of the temperature, but also the resistance which it shows to its reduction, can only be overcome by the most energetic abstraction of heat repeated time after time. In these cases we have no choice; either we must let the fever have free course, and the patient is generally lost, or we do not hesitate to push the treatment, and at the bedside to take on ourselves the consequences of what we believe to be a correct opinion. We certainly run the risk, if the case turns out unfortunately, of being blamed by the public for the fatal result. But no conscientious physician will hesitate in his course if he is once convinced of the correctness of his views.

How far we may go in this method of treatment I tested in the most striking way a few years ago, when my daughter, then nineteen months old, was affected with a serious pneumonia for the third time within a short period. The temperature exceeded 41° (105°·8 F.), and after a bath of 16° (68° F.) went up again so quickly, that I found it necessary to reduce the temperature of the bath to 5—6° (43·25—45·5° F.), and to continue the bath for ten minutes. My child recovered, and during the use of this extreme abstraction of heat, which lasted several days, there was never the least appearance of collapse. I have since then had repeated opportunities of treating patients in this way, and I have never had to regret the consequences. I have to impress on you, however, that the only precaution applies to the heart. *Allow no patient to be bathed without giving him a stimulant before and after; and increase the stimulation if the temperature of the water requires to be lowered, or the duration of the bath increased.* I have already mentioned why it is that the heart requires stimulation before the bath. And again after the bath is over, the reduction of temperature, which reaches its maximum a quarter or half-an-hour later, may, in persons with weak hearts, lead readily enough to the symptoms of collapse. Besides, it appears to me that in this way we more quickly attain to an equalisation of the temperature through the entire body.

I generally prescribe—in cases of medium intensity, which I have hitherto had in view—one or two tablespoonfuls of red wine before and after the bath. Whenever the smallest sign betrays any insufficiency in the action of the heart, then we give, instead of the red wine, port or madeira, or even champagne. If we use quite cold baths in such cases, I order one to three tablespoonfuls of red wine, to be taken five minutes before, and during the bath, and again after leaving it; and the wine requires to be stronger. In the case of children a less quantity will be used. In severe cases, however, children require proportionally more wine than adults. But it is impossible to lay down absolute rules. It is again the pulse which must decide as to when to give more, when less. Once more we have to say most emphatically: *If anyone wishes to treat cases of pneumonia with cold baths without unwelcome accidents, he must not be sparing with stimulants.*

The cases of pneumonia which are treated with warmer baths have to be considered from this standpoint also. In these cases the heart is weak to begin with, less able for exertion, and we have to expect that the disease will have a longer course. It would be very foolish at once to resort to the stronger stimulants. If we are constrained to betake ourselves to the cold bath, then of course they cannot be dispensed with. A little experience is sufficient to lead us to the proper conclusion in the actual case.

Besides the actual withdrawal of heat, I regularly use *quinine.* It possesses the inestimable advantage over the other antipyretics, that *it lowers the temperature without interfering with the heart.* And it does this, as I have found, by diminishing the production of heat. Tartar emetic, and its younger brother veratrin, diminish the temperature at the expense of the heart; they both produce collapse. When digitalis is used, the action on the heart is late of appearing. We therefore lose the command which it has over the regulation of the heart's action exactly at the moment when it might be required. Who that has had much to do with digitalis, and who has used active doses, has not seen cases in which the collapse produced by the digitalis has happened to fall in with that of the defervescence? Whoever has seen such a case will have no particular desire to meet with another. It is quite another matter to use digitalis in pneumonia as a heart regulator. Conditions of insufficiency of the heart produced by degeneration of the muscular tissue, with or without valvular disease, may make the administration of digitalis necessary, even though a croupous pneumonia may be present. I have frequently done this, especially in the case of old people. But then we give the digitalis in small, slowly assimilable doses. Its action on the temperature remains as certainly absent as its paralysing action on the heart. Why anyone in a case of pneumonia should use digitalis in order to reduce the pulse from 100 to 80, I do not understand. I do not refer to a regulation of the heart in this sense when I recommend this agent for occasional use. I willingly admit, that in many cases where the individuals are strong all these agents may be borne without injury, and in so far as they lower the temperature may be used with advantage.

And now it may be asked: Is it justifiable to use an agent which may do ultimately a serious injury to the patient, when a still better result may be obtained by means which certainly incur no such risk?

The antifebrile action of venesection is slight and uncertain. He who performs venesection for pneumonia is like the wise man who cuts down the fruit-tree to get at the fruit. The supporters of venesection furnish in the records of their own practice the best arguments for its inadmissibility, and all the more now when we have at command agents which act more certainly. At least the directions for the use of venesection should disappear from our text-books. To the weak soul whom fortune has, for the punishment of his fellow-men, made a physician, it affords a welcome salve to his conscience when, giving in to the popular clamour, he lets blood in order to make his position more secure. *Credite experto!*

When rightly used, quinine reduces the temperature during at least twelve hours. The minimum (1°·5 to 2°·5 beneath the initial height) is reached about five to seven hours after the administration of the agent. The reduction and subsequent ascent occur with almost perfect regularity, as I have determined by temperatures taken every five minutes.

The method of administration is of great consequence. I give it according to the following formula, which indicates the normal dose for an adult in an average case of pneumonia.

℞ Quininæ sulph., 2 grammes (30·8 grs.)
Acid. hydrochlor., q. s. ad solution. cum
Aq. dest., 10 gram. (2½ fl. drs.)

Sig. To be taken in one dose, between 6 and 8 o'clock in the evening.

To children I give 10 centigrammes (1½ grs.) for every year of life up to five years, after which ½ to 1 grm. (7½ to 15 grs.), according to circumstances. These normal doses may be exceeded without any evil results. We may, in highly febrile states, give an adult 5 grms. (77 grs.), and a child under one year 1 grm. (15 grs.), and always in one dose. I have done this frequently. I have slowly accumulated experience, never seen any ill results, and am of opinion that we are still far within the utmost limits of the use of quinine. I know that these doses will appear sufficiently serious to many; but experience is my mistress. Against facts only fools will fight. Whoever makes it his business to cure the sick, who does not act according to tradition at the bedside, but knows what he wishes to effect, will not shrink for a moment from acting in the same way. I have only to warn you of one thing. It is not allowable to give a pneumonic patient who has had at some time a temperature of 41° (105°·8 F.), which, after an efficient abstraction of heat, has immediately gone up again, a dose of 5 grms. of quinine right off; it is only allowable, if repeated efficient cold baths have only reduced the temperature for a short time, and smaller doses of quinine have been ineffectual; even then I have made trial of 3 or 4 grammes. I must again remind you, that the state of the heart is here the determining point. If you watch the pulse, you walk secure; if you do not, you readily stumble.

In my cases I have observed no evil effect on the heart from quinine. The pulse falls with the temperature, but remains full and strong, or it improves in character, if it has previously been not so good. The subjective discomfort is also not much greater, as a rule, with large doses than with smaller, and it quickly disappears. Vomiting occurs not infrequently a short time after quinine has been taken. If this only occurs in half to three-quarters of an hour, it is not necessary to renew the dose, as absorption is very rapid when it is given according to the above formula. Only when there is danger in delay, do we not hesitate to repeat the dose, and rather give too much than too little.

But the vomiting often occurs a few minutes after the administration. We may, to a certain extent, prevent this by directing the patient to lean forward as soon as the quinine has been swallowed, and to open the mouth so as to allow the saliva, which is generally freely secreted, to flow out. Small pieces of ice swallowed on the first appearance of nausea often prevent the occurrence of vomiting. If it occurs in spite of all, I order a second dose a quarter or half an hour later, and sometimes even a third. The second generally remains down.

If we do not succeed—as will sometimes happen—in giving the quinine in this way, then it should be administered by clyster. This almost always succeeds; the absorption does not seem to be essentially slower by the rectum than by the stomach. If quinine is to be given by the rectum, the apothecary should be directed to be careful with the acid, and to use three or four times as much water to dissolve it as that given above. If the reaction is too acid, the mucous membrane of the rectum being irritated beyond endurance, will rid itself of the inconvenient guest. A gelatinous vehicle and a few drops of laudanum are, with few exceptions, sufficient to make the gut sufficiently tolerant.

In order to have the full advantage of the individual dose of quinine, it should not be used daily, but forty-eight hours should elapse between the doses—that is to say, it should be given every second evening. By forced doses, it is possible indeed to produce a reduction of the temperature in every period of twenty-four hours. But this reduction is not so considerable as when there is a pause in the administration.

One would think that these properties would be sufficient to make every one recognise the high rank of quinine as an antipyretic. And yet we hear people often enough doubting or even denying its utility. Most frequently this has arisen from the use of too small or divided doses. The form of administration is also sometimes to blame. If we give to a feverish patient, for a long-continued period, sulphate of quinine in powder, without making the patient use acid drinks after it, we cannot be astonished if now and again it has no effect, or dyspeptic symptoms appear. The same applies, even to a greater extent, to its administration in pills. I have given up the subcutaneous injection, as it so often gives rise to abscesses.

The *diet* of the pneumonic patient will be determined by the position we hold in reference to the general question, whether we should allow a fever-patient to eat. For my own part the matter has been long determined. The idea seems to me an untenable one, according to which the administration of food is forbidden, lest it should cause a rise in the temperature by, as it were, pouring oil on the fire. It would even be untenable if proof were furnished that the supposed rise in temperature actually occurs, which it never has been. Even if it were so, I would still feed my patients; for the nutriment supplied to the organism protects its tissues from greater loss, and furnishes an equivalent for the loss already sustained. If in its oxydation heat is produced in sufficient amount to raise the temperature, we can still with the cold bath get rid of any injurious excess. But if, in order to satisfy the ravenous oxygen in the blood, it is necessary to sacrifice some of the actual tissue of the body, then a portion is wasted which may be preserved to the body. Would we rather buy wood or cut up our chairs if we were forced to keep up the temperature of our room to 25° R. (88° F.)? It is a fact taught by experience, that fever-patients who have not been starved, convalesce much more quickly; and the reason seems to me plain enough.

The only question is the proper form of nourishment. This is more easily decided if we use proper precautions to prevent the temperature from remaining high too long; for in that case we can look for much more from the patient's digestive organs. I believe that by cleansing the mouth and teeth several times a day we can essentially aid in retaining the appetite of the fever-patient. In like manner, I consider it expedient to season the food with not too small quantities of salt. Apart from its use as a nutrient salt, it also assists the appetite.

If the patient will take it, I allow him several times daily to eat some lightly-roasted finely-minced meat, with bread and butter. The best mode is to give him small quantities three or four times in the day, about an hour after a bath. I never force the patient to take this food. In any severe cases, however, I insist on him taking a strong soup, containing one or two eggs (per day), in quantities of one or two tablespoonfuls at a time. Likewise, in the course of the day, the patient is directed to take such a quantity of milk as is determined for each individual case. In dyspeptic disorders, the milk may be diluted with 1—3 parts of water; we should always see that there is not too little salt added.

I consider it a *conditio sine qua non* that the pneumonic patient should take a certain quantity of wine, varying according to age and habit. With us, Bordeaux containing about 8 per cent. alcohol is the most suitable. For adults, from half a bottle to a bottle is used daily. Whatever quantity is not used before and after the bath may be mixed with water, and drunk at intervals during the day. I also readily allow good beer. It is certain that after alcoholic drinks the temperature rather falls than rises. It is more than probable that the alcohol directly protects the tissues. The former prejudice, which chiefly concerned this point, has been shown to be without foundation, mainly by Bouviér and Binz.

It is necessary to refer here to two matters which are of considerable significance when the weakness of the heart is increasing. Neither the *pain* which the pneumonic patient feels, nor the *sleeplessness* from which he suffers, should be neglected.

There is no doubt that continued pain diminishes the power of resistance of the sufferer. In addition to this, the pain proceeding from the inflamed pleura interferes with respiration. I have found it very suitable to make hypodermic injections of morphia at the seat of pain. The dose may be a small one, generally 1 to 1½ centigrammes ($\frac{1}{7}$ to $\frac{1}{5}$ gr.) are sufficient. For similar reasons, the cough, which is often very troublesome, is to be treated. If the pain is exceedingly violent, then the quieting of it is often bound up with the necessity of obtaining sleep. We must never allow a pneumonic patient to do without sleep. Every one of us, I suppose, has had the opportunity of comparing his ability for exertion after a sleepless night with that after a night of sound rest. Such a loss of rest affects persons who are naturally weak more than the strong. But every pneumonic patient is from his illness in the position of a weak person, however strong his original constitution may be. Sleeplessness, therefore, affects him more than a healthy person. The injury to the general condition produced by sleeplessness is therefore a sufficient ground for therapeutic interference. The sleeplessness often disappears without further trouble as soon as an antipyretic treatment is instituted. If that is not the case, I use the somniferous agents in doses which, under the conditions given, will certainly be efficient. With children the antipyresis is, with very few exceptions, sufficient, and this is a circumstance which saves the physician much perplexity.

There are cases in which the whole therapeutic problem is concentrated for the time on the necessity of procuring sleep. There are cases where a condition occurs characterised by continuous delirium—often with actual maniacal outbreaks—accompanied with violent muscular movements, as well as absolute sleeplessness. Sometimes—but only in cases of imperfect antipyresis—a full dose of quinine is sufficient, quinine being considered by many fever-patients as a direct sleep producer. To this class belongs also the pneumonia of drunkards, which is characterised by the frequency of the accompanying delirium without presenting all the features of delirium tremens. And, lastly, we have delirium tremens itself occurring along with pneumonia.

No matter what may have originally induced it, the continual muscular movement demands an ever increasing amount of labour from the heart, which is often (as in drunkards) weak to begin with. The pulse soon enough betrays the fact that the heart is unable to accomplish this task. Sleep, which intermits the muscular exertion, is the only remedy. I have been able in such cases, by very large doses of the suitable preparations, to produce sleep under all circumstances. I have generally of late used chloral hydrate, in doses of 5 to 8 grammes (77 to 124 grs.), whenever smaller doses failed. We should never forget, however, in fever cases, to give before or immediately after chloral a sufficiently large dose of hydrochloric acid with suitable dilution, in order to prevent its decomposition on the surface of the stomach, whose reaction is not strongly acid or even alkaline. We should also never forget, in giving this or any other agent, to use an energetic heart stimulant. Without this, a thorough medication is not allowable; with it, we do not hesitate to go far beyond the indications of the pharmacopœia whenever there is an actual need of producing rest.*

Allow me also to state, that I always dislike the darkening of the room, and this all the more in severe acute diseases. If the bed is placed so that the light does not fall directly in the eyes of the patient, enough has been done. It seems to me that the patients recover better in the light.

We have now arrived at the *second part* of our task. It is the *treatment of the weakness of the heart which has already occurred.* In the cases which you yourselves treat from the outset, and your prophylactic measures have turned out insufficient, you will observe this rule: *The sooner you observe the approach of weakness of the heart, the easier is it to overcome it.* The reason of this is obvious. Weakness of the muscular tissue of the heart induces a retardation of the circulation. Retardation of the circulation induces a disturbance of nutrition in all the organs concerned. The longer this lasts, the more firmly established are the changes produced. If we succeed by strengthening the action of the heart in restoring after a time the normal speed of the blood-current, it still requires a period of time, proportionate to the duration of the disturbance, in order to make good the injury already done. This injury will of course be greatest in those parts which have most need of blood. And these are the working muscles—the heart and respiratory muscles. I attach great weight to this circumstance, and therefore pay continuous attention to the first symptoms which indicate a deterioration of the heart's action. I am of opinion that I owe my success at the bedside in no small degree to this. Besides the character of the pulse, we must carefully watch the relation between its frequency and that of respiration; and lastly, the occurrence of cyanosis, which very soon shows itself. The temperature shows no constant difference in the first stages of collapse.

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* Compare my communication, "Ueber die Behandlung des Säuferwahnsinns." Deutsche Klinik, 1872, Nov. 11.

There is often nothing more than an indication of the approach of collapse. I suppose that these slight disturbances in the course of the disease, which are met with in almost every individual case, are most frequently associated with a local extension of the disease. This is by no means always indicated by the thermometer. The judgment of the physician requires to be exercised, whether he will interfere or not. In strong persons, I prefer not to do so till the phenomena are more distinct. I am aware, however, that we may initiate our treatment even earlier without injury.

The picture of a fully-developed collapse, with its most prominent symptom, œdema of the lungs, is so characteristic, that its features will be ineradicably impressed on every one who has seen it. That we have here the beginning of the end, is apparent to the dullest eye. Every advanced state of collapse leads certainly to death if the physician does not come to the rescue. Anything that is now attempted must be capable of doing great things speedily. But what is to be attempted?

Here we have a difference of opinion of the most decided significance. The importance of the matter demands that we go into it more in detail.

I have hitherto been continually speaking of weakness of the heart, but have not yet mentioned how its effects show themselves.

We have already seen that in pneumonia the right ventricle is in the first instance weighted. If the heart gives in, then the first disturbance shows itself in this part. With diminished action of the right ventricle there occurs stagnation in the domain of the pulmonary circulation and emptiness of the left ventricle. In the higher degrees, on account of the difficulty of exit from the right ventricle, which is never properly emptied, and on account of the diminished vis a tergo, there is accumulation of the blood in the veins of the systemic circulation. In more strongly developed states of insufficiency of the heart, œdema of the lungs may show itself. In fatal cases this is never absent. The œdema of the brain, which has been much discussed by authors, and is produced by stagnation in the region of the cava superior, may occur as an exceptional rarity. The symptoms described as œdema of the brain may be equally well viewed as those which are produced by high fever along with diminished aëration of the blood. Similar complications occur—just as seldom as in pneumonia—in all severe febrile affections. The *emptiness of the left ventricle,* however, has always, and under all circumstances, very injurious results; the active muscles, the heart, and respiratory muscles do not receive a sufficient quantity of blood. Their power of action is thus seriously menaced. It is therefore more correct to look on the insufficiency of the heart as the cause of death in pneumonia than the customary œdema of the lungs.

This is more than a change of name. If we speak of the œdema of the lungs, we readily picture to ourselves a purely mechanical obstacle between the blood and air from the serum poured into the alveoli; the evolution of carbonic acid will in this way be interfered with, and, being retained in the blood, gradually tend to death. It is certain enough, that the filling of the alveoli with fluid hinders aëration. But, besides this, it must be remembered, that in consequence of the retardation of the circulation which precedes the occurrence of the œdema—a retardation which is due to weakness of the heart—nutrition is delayed in the tissues as a whole, and a retention of all the products of oxydation in the tissues takes place. We ought therefore in our nomenclature to indicate not the result but the exciting cause, and this is the insufficiency of the heart.

Within the last ten years a view has been brought forward, which differs very much from the above. Niemeyer speaks so constantly of an œdema of the lungs, which is produced by the collateral fluxion in pneumonia, that the reader who is unacquainted with the facts is led to the view that it is a common occurrence.

This doctrine is based on the following considerations. The closure of a portion of the outlet-tubes in the region supplied by the pulmonary artery, which is necessarily produced by the pneumonic exudation, calls forth a fluxion, that is, an increased and accelerated flow to the rest of the lung-tissue. The pressure of the blood acts on the capillaries of the lung, which are not, is in other organs, "imbedded in a more or less resisting tissue, but lie with the greater part of their wall almost or quite unsupported in a cavity containing air, and air which is rarefied at each inspiration"; they are therefore not in a condition to withstand an increase of the blood-pressure. They are accordingly first dilated, and then they allow their contents to escape.*

A result of this view, which has been made use of by Niemeyer in a therapeutic sense, is that a strengthening of the action of the heart will lead to an increase of the accumulation of fluid thus arising in the alveoli. It is easy to prove that this deduction does not agree with the facts.

1. If this roughly mechanical relation were applicable, why is it that we have not collateral œdema in every case in which the patient is a strong individual? Why do we meet with œdema of the lungs in the weak and not in the strong?

2. In emphysema of the lungs, which is treated of by Niemeyer exactly from the same point of view—that of diminu-

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* Niemeyer goes even farther; he believes that increased action of the heart alone may produce hyperæmia and œdema of the lungs.

tion of the number of the capillaries—we have constantly hypertrophy of the right ventricle, therefore an increase of the vital force furnished at each contraction, and so an increase of the pressure in the capillaries of the lung already overfilled by "collateral fluxion." And yet œdema of the lungs does not occur as long as the right ventricle is hypertrophied, but only when it presents fatty degeneration of its muscular fibre. How does this agree with the above mode of explanation?

3. We never find during œdema of the lung an exaggeration of the heart-sounds over the right ventricle or pulmonary artery.

4. I have myself observed about a thousand cases of pneumonia. Among these there is one, so far as I remember, who twice presented œdema of the lungs twenty-four hours after the invasion of the disease. All the others did so later on. This one case was treated with the most powerful stimulants, and recovered. I am not able to give positive numbers, but I dare trust my memory somewhat, as I have paid special attention to this point for some years.

I remember very distinctly two cases of pneumo-thorax, in which, a few hours after the occurrence of this condition, paralysis of the heart and œdema of the lungs appeared. In this case also, where the collapse of an entire lung offered the most perfect opportunity for the development of a "collateral active œdema," this disappeared after the use of the strongest stimulants, given dose upon dose. The experience of one person is like a drop in the sea. But how many physicians see œdema of the lungs in the period of pneumonia when the local changes are being established? How many of us see œdema in pneumonia along with exaggerated action of the heart? I think I may leave the subject without further discussion. If we cannot theoretically establish the impossibility of an œdema arising in this way, experience teaches us at least that its occurrence is wonderfully rare. If it occurs, there must be, along with the physical peculiarities, also a physiological one, namely, diminution in the power of resistance of the vessels, as in those predisposed to phthisis, in drunkards, etc. If the purely mechanical relations of which Niemeyer avails himself, are the most important, then we should necessarily find that œdema occurred more frequently at the onset of pneumonia. But if the œdema is a passive one, due to the fact that the heart is unable to overcome the obstructions which lie in the way of the current, then what will happen? The schools teach, that a diminution of the mass of blood as a whole leads to a diminution of the work required of the heart, as the mass to be moved is less. Simultaneously however there occurs absorption from the tissues, that is to say, from the surfaces of the internal organs, during which the fluid effused into the lung-alveoli is again taken up. Venesection thus diminishes the labour imposed on the heart, and causes the œdema of the lungs to disappear by absorption. Venesection is therefore thoroughly indicated in passive œdema of the lungs. Experience teaches that, as a matter of fact, the most serious phenomena recede after venesection under the conditions described. Often very rapidly—so that there are few agents which are able to produce a more striking result.

And yet, is this agent the really judicious one? or are there others which can produce the same result? If blood-letting were an unequivocal agent, then all discussion would be at an end. But it is perhaps the most equivocal agent of all. What generally happens when in pneumonia venesection has been performed on account of pulmonary œdema? In twenty-four hours, often much sooner, the condition is the same as before the blood-letting. The lancet is again used, again the symptoms disappear. If the crisis, the natural turn of the disease, does not very soon occur, then the patient certainly succumbs to a third or fourth bleeding, which has become necessary. Even at the first blood-letting we have to bear in mind that the interference is only resorted to in order to gain time for the occurrence of the spontaneous termination of the disease. And I would myself, *if every other possible means were cut off*, of relieving the patient, let blood in the individual case, in order to gain time. But may we not reach the same end by a less dangerous way. If it were possible to excite the heart to increased exertion for a time till the obstruction in the pulmonary circulation was overcome, then the problem would be solved. Experience must, therefore, decide whether the increased action is not quickly followed by exhaustion to such a degree, that results will be produced in the heart as bad as those from blood-letting.

Let us consider the principles involved in these two methods.

Every working muscle requires oxygen, and the more oxygen the harder it works. *Cæteris paribus*, the amount of oxygen carried to the muscles is dependent on the amount of the red blood-corpuscles. This latter amount is diminished by blood-letting. After venesection, therefore, less oxygen can be taken up by the blood. If the necessities of the system exceed the amount contained in the blood, then the needful increase can only take place by increasing the speed of the circulation. The number of the red blood-corpuscles must be looked on as constant for the period of time involved, and therefore the only possibility remaining is that in a given time the contact between blood and air in the lungs for taking up oxygen, and the contact between blood and tissues for giving it off, must be increased. In addition to this, the diminution of the weight to be moved by the heart, which the bleeding at first brought about, is of very short duration, for the volume of the blood is quickly restored by absorption from the tissues. To put it in one sentence, this means that:

Heart and muscles of respiration must work more vigorously after blood-letting than before it, in order that the same quantity of oxygen may be supplied to the tissues of the body.

The need of oxygen by the body, however, will only be reduced by the bleeding, if it produces a considerable diminution of temperature, and only so long as this diminution lasts. As a rule, the need of oxygen is as great after as before the blood-letting, or even greater; hence the evanescent improvement which blood-letting produces is easy to understand. Only the diminution of the demand for oxygen—the crisis—produces a diminution of the internal muscular exertion. Venesection is thus a very equivocal helper in our necessity. It is like the usurer, who willingly lends sum upon sum on good security, in order at last to take possession of the whole of his debtor's property. For the moment, the loan causes the embarrassment to disappear; but if there is no diminution in the outlay, the catastrophe is only a matter of time.

I must refer to an objection, but only an apparent one. How does it come about, if the above arguments are correct, that immediately after the bleeding the heart and muscles of respiration work more strongly than before? That happens because the right ventricle is for the moment so lightened of its weight, that it is in a position to propel more blood to the left. If the left ventricle, however, receives more blood after the bleeding than before, then the heart and respiratory muscles receive an increased supply of oxygen with the increased supply of blood.

It is very different in the case of stimulants. Here the muscle of the heart is spurred to absolutely more exertion; but it is placed in a position by means of the agent used to overtake this work. Every powerful systole of the heart, pressing more blood from the over-filled right ventricle into the left, actually brings more oxygen to the heart, removes the accumulated products of oxydation, and is of assistance in the first place to the heart itself. The mass of the blood is undiminished; it is only necessary for a short time to call forth an increased exertion in order to be relieved of the momentary difficulty. Under these circumstances, the stimulant is in fact not only whip, but provender for the heart. For it furnishes it with oxygen, without which muscular labour is impossible.

There is no doubt as to the decision to which this theoretical reasoning leads. If the way indicated is practicable, it is to be preferred. And experience teaches that it is so.

It is in fact possible, by the proper and bold use of stimulants, to keep a pneumonic patient in life for at least three or four days after weakness of the heart has shown itself. We are often able to keep the patient even longer.

If slighter degrees of weakness of the heart are present, then in the majority of cases a full dose (150 grms. = about 4·8 ounces) of a strong wine (port, madeira, sherry, etc.) is sufficient to make it disappear. The size of the dose, and its frequency, are of course to be determined by the peculiarities of the individual—chiefly his greater or less customary use of wine. If these slighter attacks occur more frequently, then I soon order a camphor emulsion (3 grammes [=46 grs.] in 200 of water; a tablespoonful every two hours). If the weakness of the heart continues, then I order a tablespoonful of strong wine and camphor emulsion alternately every hour or half-hour. Sudden collapse of considerable intensity is suitably met by musk (50—100 centigrammes [8 to 16 grs.] for a dose) with one or two tablespoonfuls of champagne. We give the agents every ten minutes to every half hour till improvement appears. Musk and champagne act rapidly, camphor more slowly, but its effect remains longer. In severe cases therefore, with frequent occurrence of weakness of the heart, I generally give camphor for some time after the musk and champagne, in order to retain the advantage gained. In patients who are unable to swallow, and cannot retain a clyster, I have of late used frequently subcutaneous injections of camphorated oil (one part of camphor to nine of olive oil). We inject, by means of the hypodermic syringe, sufficient to incite the activity of the heart; and the best locality seems to be the subcutaneous tissue of the thorax. Its effect soon appears. Abscesses do not occur. Perhaps still more rapid than musk with champagne, is the action of hot toddy; 1—2 parts of cognac or rum—if nothing else can be got, even schnapps [say whisky]—to 1 part of water, or strong coffee or tea. I order one or more tablespoonfuls of this to be given every ten minutes. We often see remarkable effects from this treatment. But here again the action is of short duration; means must be used to establish it. In the case of persons with weak hearts—especially old people with very atheromatous arteries—you should give, on the day when the crisis is expected, as a matter of prophylaxis, half a gramme (8 grs.) of musk in the evening. In this way collapse will be often prevented.

To construct a scheme for the use of stimulants would be a very thankless task. Let us make our own the axiom according to which we shall use them at the bedside, and we shall soon enough, with quiet observation, attain to certainty. Early therapeutic attention to the first symptoms generally renders the use of the heavier artillery unnecessary. But if these require to be drawn into the engagement, we must be impressed with the rule that it is not possible to give an upper limit for the dose of stimulants; it is necessary, when the weaker stimulants fail, to resort to the stronger, and increase the dose. In order to attain our object, bold and consistent action is all that is necessary; whoever is faint-hearted in the decisive moment is much out of place at the bedside.

The supporters of blood-letting have mostly already come so far as to give stimulants *after* the venesection. Within certain limits, no doubt, something is to be attained by this practice. But we should not hesitate to go the whole length—to let go the bleeding, and to treat the pulmonary œdema from the outset with stimulants. The first trial of this method will not be the last.

Should we or dare we use the bath, when the symptoms of weakness of the heart are unmistakeably present? Theoretically, the answer is this: the bath increases temporarily the labour imposed on the heart, but diminishes the work to be done by it as a whole. If we have not to do with the most severe collapse, then we can, with sufficient care in the selection of stimulants, and of the temperature of the bath, treat most patients very well with direct abstraction of heat. All bear large doses of quinine. I have been gradually advancing under the teaching of experience. Whoever has confided in these axioms in the treatment of pneumonia, and has become accustomed to a low mortality, will proceed more boldly than one who has first to collect his experience. We should never forget that the most dangerous enemy of the heart is the high temperature, and this is certainly and quickly diminished by the bath.

I must call your attention to one circumstance more. Not at all infrequently there occurs in weak individuals just at the time of the crisis, or even a few days after, a sudden collapse. This generally passes quickly and spontaneously away; sometimes, however, it is accompanied with danger. I think that this collapse is less frequently met with when we allow the use of a light wine for five to six days after the defervescence. We should prepare the attendants for the possible occurrence of this, and give our directions beforehand, for here rapid treatment is necessary. As you have seen, we seek to make the convalescence of our pneumonic patients as short as possible, by making the nourishment sufficiently rich—chiefly with albuminates—and with sufficient quantities of beer or wine. We gladly avail ourselves also of preparations of iron. You know the form in which we give this agent to dyspeptics:

R. Ferr. redact. 8 grm. (127 grs.).
Extr. Cinch. reg. frigide parat. 2 grm. (30 grs.).
Div. in pilul. Nr 100.
Sig. Three pills to be taken thrice daily, ten minutes after food.

We begin with one pill, and increase it gradually, if there is serious dyspepsia.

If the absorption of the exudation is delayed, I cannot strongly enough recommend the use of oil of turpentine. I order generally twelve drops, to be taken six times a day in milk or capsules. I have not seen any advantage from larger doses—though we can give much larger ones without injury. If a considerable quantity of milk (100—150 cm.) is taken with each dose, there is seldom any gastric disturbance.

The view advanced in these pages has, to begin with, the advantage that it developes the whole subject of the danger to the organism from pneumonia, from one consistent point of view, and makes clear the necessary rules of treatment. But in matters of therapeutics, the result is the only judge; every conclusion, be it ever so enticing, is worthless if not supported by experience at the bedside. I can assert that my views have not arisen at the desk, but by the bedside, and that my theory is the child of actual practice.

But by its fruits shall you know it, and I therefore lay before you in conclusion, not statistics, but a simple collection of the last 200 cases treated at the Poliklinik, according to the axioms which I have introduced to you. There is here no selection, but case after case as it follows in chronological order in our records. Statistics of pneumonia, in order to lay claim to any value, must, as it seems to me, be purely post-mortem statistics; otherwise there is too great room for accident, unless we have thousands of individual cases at command. Examine this table without prejudice, and I am convinced that you will say with me, that therapeutics has done all that you could fairly expect from it. I consider it right to include every fatal case which has been diagnosed as pneumonia at the bedside, and confirmed as such by post-mortem examination. This excludes all arbitrariness. If I, on the other hand, were to place quite roughly 400 cases treated at the Poliklinik before these axioms were acted on against other 400 treated since then, it would appear that the mortality has diminished by half. But these numbers are still too small. My post-mortem statistics are my most powerful arguments.

AGE.	SEX.		DEATHS.	REMARKS ON THE FATAL CASES.
	M.	F.		
0 to 1 year	6 4·5 per cent.	3	0	
1 to 5 years	34 31·5 per cent.	29	2	*a.* A boy 1¼ years of age; terminal pneumonia in follicular catarrh of the bowel. *b.* A girl 1¼ years of age. Terminal pneumonia in multiple circumscribed gangrene of the skin. The most horrible lodging in a cellar.
6 to 10 years	20 19·0 per cent.	18	2	*a.* A girl of 7 years. Miliary tuberculosis after pertussis, with principal seat in meninges; finally pneumonia. *b.* A boy of 6. Pericarditis complicating a left side pneumonia, which had involved the tongue-shaped process. Operative removal of the purulent exudation. Death with meningitis cerebralis.
11 to 20 years	10 8·0 per cent.	6	0	
21 to 30 years	5 6·5 per cent.	8	0	
31 to 40 years	11 7·0 per cent.	3	2	*a.* A woman of 33 years. Abortion, with incontrollable hæmorrhage. Death with symptoms of acute anæmia. *b.* A man of 33. Terminal pneumonia of diabetes mellitus.
41 to 50 years	11 7·5 per cent.	4	8	*a.* and *b.* Men, with delirium tremens, of 46 and 50 years respectively. *c.* A man of 49, with extreme emaciation from drinking. *d.* Woman of 42; abortion, with profuse hæmorrhage. *e.* Woman of 46; very much emaciated (vagrant). *f.* Woman of 45; acute rheumatism; pericarditis; complete adhesion of both pleuræ. *g.* Man of 46; epidemic cerebro-spinal meningitis. *h.* Man of 47; died a few hours after admission; post-mortem: pneumonia, with purulent infiltration.

AGE.	SEX.		DEATHS.	REMARKS ON THE FATAL CASES.
	M.	F.		
51 to 60 years	9	5	2	*a.* Man of 54; a gross drunkard; probably complicated with epidemic cerebro-spinal meningitis. *b.* A man of. 55; delirium tremens.
	7·0 per cent.			
61 to 70 years	6	4	4	*a.* Woman of 69. *b.* Man of 70; pneumonia of doubtful significance, with heart disease and hemiplegia (embolism). *c.* Man of 64; total obliteration of both pleuræ. *d.* Man of 69; terminal pneumonia in cancer of stomach.
	5·0 per cent.			
71 to 90 years	2	5	4	*a.* Woman of 74; terminal pneumonia in peritonitis chronica. *b.* Woman of 81. *c.* Man of 77, with highly atheromatous arteries; pulse 80; temperature 39° (102°·4 F.). *d.* Woman of 79; intense atheroma, with mitral insufficiency.
	4·0 per cent.			
93 years	0	1	0	
TOTAL	114	86		
	200		24	

The individual cases are still in your memories, gentlemen; how you stood anxiously by the bedside, when a severe pneumonia threatened the life of one confided to your care, and you then saw how your interference, apparently so inconsiderate, removed the danger from the sufferer. I need not tell you that my therapeutics are not heroic. You have formed your judgment by the bedside; that is sufficient for me; I am content to await the issue.

Allow me still to direct your attention to one point. My directions as to the treatment of croupous pneumonia are not to be looked on as a method, which must be used always, and under all circumstances. I have attempted nothing more or less than to find that danger which is the chief of those which threaten the life of a pneumonic patient. This danger may be of so little importance in a given case that it may be set aside; and, as a matter of fact, it is so in the great majority of cases of pneumonia in childhood and in years of vigour. Such cases require, as a rule, no treatment in order to recovery. The antipyretic mode of treatment may then be taken up as a prophylactic measure in the sense which I have developed.

Be that as it may, each individual patient is a problem by himself. Individualisation is necessary in pneumonia in a very much higher degree than, for example, in that other disease treated by abstraction of heat, typhoid fever. One thing must in future be observed. Any weakening mode of treatment, any agent which in addition to its antipyretic action has a weakening effect on the heart whenever it is given in doses sufficient to lower the temperature, should not be used in the treatment of pneumonia. In the same way any method of treatment which from the outset spurs the heart to too rapid action is bad; for exhaustion follows on overwork.

In the given case, the right use of the proper principles of treatment shows whether one deserves the name of a true physician.

ON PUERPERAL PARAMETRITIS
AND
PERIMETRITIS.

BY

PROF. R. OLSHAUSEN,

OF HALLE.

GENTLEMEN,—We will to-day take a retrospective view of the course of the disease in two lying-in women, who, as you have observed, have both been suffering from an inflammatory affection of the lower part of the abdomen.

The one of them, Fr. Kömmling, has lately been delivered of her fifth child. She is 32 years old, and has now reached the eighteenth day of the puerperal state. The confinement this time ran an entirely normal and easy course. After the water had broken, two forcing pains were sufficient to bring the child into the world. On the first day of the confinement she felt perfectly well, and the pulse, as is the rule in normal cases, had lessened to 58 beats. But in the course of the second night she had a severe rigor. On the following morning we found her suffering from excessive thirst and headache, with a full, bounding pulse of 108 beats in the minute. The temperature was 39·8° C. Violent abdominal pains, partly of a stabbing, partly of an aching character, had prevented her sleeping the whole night, and these still continued. The uterus was large and sensitive, particularly on the right side. On the same day not only did the whole womb become tender, but this tenderness extended, in the neighbourhood of the right margin of the uterus, into the right hypogastric region. The application of ten leeches quickly alleviated the pain, but did not prevent its return. The tenderness on pressure remained considerable, especially on the right side. There was no swelling at any time of the external genitals. The lochia were normal in quantity and quality, or were only slightly lessened on the second day of the illness. Sleep was bad.

On the sixth day not only the abdominal pains, but the tenderness on pressure had almost entirely disappeared; the evening temperature was reduced to 38° C., and the pulse to 68 beats. This satisfactory state of health was, however, not of long continuance. After a bad night, during which she had had almost constant rigors, and had once vomited, we again found her on the eighth day with severe pain, chiefly in the right side of the lower part of the abdomen. There was great sensitiveness to the slightest touch. Blood was again drawn.

On the tenth day an increased resistance towards the left side was perceptible through the abdominal walls to the right of the uterus; and two days later a distinct tumour, which lay close to the uterus, but yet did not deaden the resonance on percussion, as it was not in immediate contact with the abdominal walls, but at some distance from them.

After the eighth day the fever had again become continuous, and was higher than in the first attack of inflammation. The evening temperature had repeatedly reached 40·6°; the pulse varied between 88 and 112.

On the twelfth day the pains and the tenderness were decidedly relieved. The internal and bimanual exploration which was first undertaken on the fourteenth day, showed that the uterus was full, large, and only slightly moveable. No exudation could be felt per vaginam, only an increased resistance was perceptible to the right of the cervix uteri. From the fifteenth day she has had no fever; and from the sixteenth day until now, the eighteenth, the temperature has been reduced—the best sign of commencing re-convalescence. In the last few days the evening temperature, too, has not exceeded 36·8° C. The pulse is between 50 and 60. The exudation is already decidedly less, and is less near the abdominal wall than before.

These are the principal features of the illness which has just passed, and which we recognise as perimetritis on account of the severe pains and of the great and long-continued tenderness, both symptoms pointing to implication of the peritoneum, even if the evident extra-peritoneal exudation puts beyond question the fact of the simultaneous participation of the subserous cellular tissue in the inflammation, the so-called parametritis. We can find no cause for the relapse which occurred after two days' absence of fever (on the sixth and seventh), and which exceeded the first attack in duration and violence, just as we know no cause for the origin of the disease in the first place. After the rapid and easy labour with this fifth child there could be no question of the contusion of the soft parts, or of lesions of the vagina and vulva. The whole time of the illness, from the second to the fourteenth day of the lying-in, has somewhat exceeded the usual extent of these affections.

The illness of the second lying-in woman assumed in many respects a different appearance. Twenty-one days ago she was delivered of her first child; the labour passed off without any irregularity, the head presenting in the first position. There was a not very shallow laceration on the left side of the lower part of the vagina, as is usually the case with primiparæ.

On the second day of her lying-in she complained, notwithstanding its being her first child, of violent after-pains, and the uterus was sensitive on pressure. In the evening the temperature had risen to 38·6° C. There were no rigors. The fever soon increased considerably, reached its height on the sixth day, with a temperature of 41·2, continued very high till the fourteenth, mostly with an evening temperature of 40·5° C. or more, was only decidedly lessened on the fifteenth day, and was at an end by the seventeenth. During the whole of this time the pulse numbered from 90 to 108 beats; only once were 128 counted. In the beginning of her illness she complained a good deal of headache, sleepless nights, and thirst, but of no abdominal pains. On the fifth day, however, there was tenderness in the whole of the lower part of the body; the left labium majus was swollen; the lochia were offensive.

On the eighth day the tenderness was concentrated more on the left border of the uterus; there was also a moderate degree of pain. The sensitiveness to pressure continued, chiefly on the left side, but was not considerable. On the thirteenth day an increased resistance was perceptible on the left side above Poupart's ligament. An internal examination showed that the laceration of the lower part of the left side of the vagina was cicatrising; the place was, however, still tender; the left labium majus continued swollen. The portio vag. uteri was very high, and at the same time so short, that little could be made out as to the mobility of the uterus. On the left side of the portio vag., and rather forwards, a firm, immoveable swelling could be felt through the vaginal wall, which became more distinctly marked in the next few days, forcing the vagina on the left side, not inconsiderably downwards; it extended outwardly to the side of the pelvis, and internally to the uterus, appearing to form a single mass with both parts. On the ninth day repeated action of the bowels set in, with thin, fæculent stools, without any symptoms of irritation of the rectum; this was never profuse, and was only accompanied by occasional intestinal rumblings.

Later on the tenderness on the left side diminished; for a few days the fundus uteri and then the right edge of the uterus were painful. On the sixteenth day, on examining per vaginam, an exudation could be felt on the right side in the neighbourhood of the womb. The general state of her health, in spite of these appearances and of the severe fever, was, except at the beginning, not materially disturbed. Her principal complaints referred to the want of sound sleep and of appetite. At present, on the twenty-first day, we find that the temperature has during the last five days been constantly below 37·5° C.; the pulse has 60 to 64 beats in the minute. The fundus uteri still reaches considerably above the symphysis pubis, and can consequently still be felt by an external examination alone, owing more to an abnormally high position of the organ, than to any defective involution. The lateral regions of the lower part of the abdomen are still somewhat tender on deep pressure. The exudation is no longer externally perceptible, unless indistinctly on the left side on examining bimanually. Internally, absorption appears to have commenced in the exudation on the left side, while that on the right presses the vaginal wall strongly downwards.

We have defined the affection in this case as parametritis, because the symptoms of peritoneal inflammation, if not altogether absent, were at any rate not very pronounced; for the pain was very moderate and did not last long, while the tenderness on pressure was but for a short time of any considerable intensity. Vomiting was wanting, as well as the premonitory shivering. But the exudation in both ligamenta lata uteri proves most clearly the situation of the inflammation. The locality of the inflammation was principally in the pelvic cellular tissue.

We can connect the origin of this phlegmonous affection without difficulty with the important, deep-seated laceration on the left side of the vagina, which at the same time gave rise to a phlegmonous swelling of the vulva of that side, which lasted fourteen days. The inflammation then crept up in the paravaginal cellular tissue, as high as the left border of the uterus, and later spread to the right border. The temporary offensive lochia were owing to the ulceration in the vagina, the secretion from which set up further decompositions in the lochial discharge. We are not justified in assuming on this account an affection of the inner surface of the womb. Absorption of the existing exudation has already commenced, on the left side at least, where it first appeared, and absorption will probably be complete in the course of a few weeks.

If, Gentlemen, we have separated these cases from one another as perimetritis and parametritis, it need naturally not be inferred from this distinction that in both cases the peritoneum as well as the subperitoneal cellular tissue was not affected; only in the first-mentioned case the affection of the peritoneum preponderated, and therefore the name perimetritis was chosen. Both tissues are nearly always implicated, and it may easily be said that for many cases the one name is as good as the other. Yet there are many puerperal women in whom the affection of the cellular tissue is at least not evident; and on the other hand, even if much more rarely, there are cases of nearly or quite pure phlegmonous inflammation of the pelvic cellular tissue without implication of the peritoneum. Were we to employ the expression parametritis for these cases, it could only be seldom used. In the great majority of cases which are so called, there is, at least at the commencement, very decided peritonitis. Mostly, however, after a few days the peritoneal symptoms recede, and whilst the pains are mitigated or cease, and the tenderness remains but slight, the fever continues. The woman then feels herself subjectively well for some time to come, because she has no pains, although the evening temperature may still show from 40° to 41° C.

If, Gentlemen, we now investigate the causes of the illness in the two cases in question, you will perhaps be struck by the circumstance that two lying-in women should have been so extremely ill after perfectly easy and successful labours, while we see so many escape all inflammation after much more difficult labours accompanied by long-continued expulsive pains. If the etiology of puerperal inflammations is not *à priori* rightly understood, as may easily be the case with a beginner, but is judged of from facts, we soon learn to perceive that the difficulty of the labour, and the bruisings and lacerations arising during the course of the labour, must not be taken as the principal causes of the inflammations, but that in the majority of those affections they are of little or no account. Naturally we would not deny that there may be traumatic puerperal inflammations in the above sense, or even that the bruisings of the soft parts in the vaginal canal may be alone sufficient to give rise to peritonitis or parametritis. In those cases where the expulsive stage of labour has been slow and difficult, where unfavourable circumstances have occurred, to which bruising might be imputed, as premature rupture of the membranes, narrow pelvis, rigid os uteri, or operative interference where the bruising was visible and the inflammation commenced soon after or during the birth, and preserved the character of a local affection—there we have to think of the above-named mode of origin, but we may not forget that, in such cases especially, other circumstances exist which favour inflammation.

Such a point of origin is especially formed by the wounds arising in the genital canal, even in the easiest labour. Its two narrowest parts, the lowest segments of the womb and the vagina, hardly ever remain quite intact during the passage of the child. The upper half of the cervical canal certainly receives mostly only very unimportant injuries, which are limited to a loss of epithelium. But the os uteri and vaginal orifice exhibit after nearly every birth, notably in primiparæ, tolerably deep injuries extending into the submucous cellular tissue. If we agree with Friedländer, we have the whole inner surface of the uterus, from the orificium internum upwards, denuded of its epithelium, which is thrown off with the decidua vera at the commencement of the puerperal state, and we must therefore represent it to ourselves as a single open wound in the cellular tissue, well fitted for the absorption of the secretions of wounds. At any rate, the other wounds already mentioned offer plenty of opportunity for the absorption of portions of those fluids with which they come in contact. In this manner, inflammations are directly excited in the neighbourhood of wounds, which find in the cellular tissue the most suitable soil for their rapid spread and final extension to the peritoneum.

That the lateral lacerations of the cervix uteri are thus very frequently the point of origin of the parametritis and perimetritis, without regard to the size and depth of these injuries, is probable, because most inflammations commence at the side of the cervix uteri. In the majority of inflammations, the one or the other lateral edge of the uterus becomes most quickly tender in its lower segment, and most exudations establish themselves in immediate connection with the cervix uteri in the inferior parts of the lig. lata. Together with the wounds of the cervix uteri, those of the introitus vaginæ have to be considered, and these, as is well known, have usually a lateral situation. They very frequently cause those well-known swellings of the vulva, by which the surface of the wound, at first angular and sharply defined, is changed into a less circumscribed and rounder sore, the surface of which looks unclean, and is not uncommonly covered with a dirty-yellow, fatty mass; there is also some tenderness of the œdematous swelling in its immediate neighbourhood. Not unfrequently, one or more days after the first appearance of the phlegmonous process in the vulva, we see perimetritis developed on the same side on which was the wound of the introitus vaginæ which had caused the phlegmon in the vulva. In such cases we occasionally see fever of one or more days' duration precede the first development of peritonitis, or the first appearance of tenderness of the uterus and its neighbourhood, and this fever depends on the gradually advancing phlegmonous process in the paravaginal tissue. We may assume such a connection in the second of our cases between the deep laceration of the introitus vaginæ and the parametritis which commenced on the same side.

So much concerning the local origin of the inflammations. But, we must ask, what are the materials which, being absorbed, directly excite inflammation around the wounds, whence it makes its further progress? The secretion furnished by the not in general very extensive surfaces of the wounds is of the least consideration. There is always another fluid, loaded with the *débris* of decayed tissues, which irrigates all the wounds in the genital canal—the lochial discharge. The peculiar smell of the normal lochia even points to the decomposition going on in the fluid, and the cases of so-called lochiometra, where by an accidental closure of the cervix uteri the lochia have been dammed up, and in consequence fever has arisen, prove almost with the certainty of an experiment that the lochia possess materials with pyrogenetic properties. Hence the phlogistic quality of the lochia becomes very probable. The secretion from wounds of the genital parts and other influences, as probably the entrance of atmospheric air into the puerperal uterus, often excite further decomposition of the lochia, which then get to smell putrid, and certainly act so much the more perniciously. At present, I have not yet taken into consideration certain abnormal conditions, as the retention of the remains of ova, the putrefaction of a dead fœtus in the uterus, or the decomposition of blood-coagula. The products of putrefaction are here developed, which becoming absorbed, give rise to the so-called self-infection (in the more narrow sense) of lying-in women, and cause severe septicæmia as well as inflammation.

A larger rôle, than by this form of infection, is undoubtedly played by the infection which is conveyed to puerperal women from external sources. That products of animal decomposition of the most varied kinds, cadaveric poison, cancerous ichor, the ichor of phlegmonous erysipelas, as well as every possible form of contagious distemper and decomposed abortive ova may furnish these dangerous products, is well known to you; also that the poison may be conveyed by the examining finger, by the linen, or by instruments. Every wound of the mucous membrane of the genital canal can take up this poison, but in all probability it most frequently occurs at those parts of the cervical canal which have lost their epithelium. It is a question whether the intact mucous membrane is capable of such absorption.

I have thus, Gentlemen, come to speak of the etiology of puerperal septicæmia, although I have no intention of going further into it to-day. But I could not quite pass it over, on account of its close connection with puerperal inflammations. This connection exists in more than one respect; principally etiologically, as already pointed out. Every wound, especially those penetrating the mucous membrane, further, every abnormal decomposition of secretions, and everything that favours the reception of materials into the circulation, promotes the accession of phlegmonous inflammations, as well as of general septic affections.

That the conditions for absorption on the part of a wound of the puerperal uterus must be unusually favourable, becomes very probable from the extreme richness of the organ in blood-vessels and lymphatics, and particularly from the physiological fact of the very rapid involution of the organ in the course of a few weeks. This process, which has certainly no parallel in physiology, and hardly in pathology, is not conceivable without an extraordinarily increased absorption from the parenchyma; in other words, in consequence, probably, of the suddenly altered conditions of the circulation and of the pressure in the vascular system that occur after birth, there results such an enormous passage of material into the venous and lymphatic vascular system of the uterus, as takes place at no other time.

If this passage of parenchymatous fluids be facilitated, it will most likely be the same for those fluids which are found on the surface of the mucous membrane, or on wounds of that membrane. This partly explains the facility with which inflammations and septic processes arise in puerperal women, owing to the absorption of deleterious materials.

But you will ask, is there no difference of an essential character in the kind of deleterious matter absorbed, with respect to the development of either form of disease? Most certainly such differences exist. There are matters, as experiment, and many cases of puerperal septicæmia prove, the inoculation of which in the most minute quantities is sufficient to produce the most severe general epidemic affections, while, on the other hand, the normal lochia, the normal secretions of wounds, good pus, whose capacities for exciting fever and inflammation we must vindicate, do not evidently contain this kind of poison. Consequently it has been proposed to distinguish septicæmic, ichorrhæmic, pyæmic, simple traumatic fever, etc., according to the quality of the poisonous material. But such a division is only an endeavour to cloak our ignorance. Certain as we may be that these respective materials must be of very different character, we, as yet, know nothing of their nature, in what fluids this or that one is to be found, nor in what way they act individually.

The still unconfirmed results of Bergmann, who believes that he has discovered in Sepsin a definite material that may be produced in an isolated form, and possessing definite properties, would only form the commencement of our knowledge as to the nature of these various deleterious materials.

If it be difficult theoretically, it is still less possible practically, to arrive at a satisfactory analysis of the morbid processes, or at a division of the inflammations into such as are purely local affections, and into such as may be called septic. There certainly exist great and easily recognised distinctions between a limited perimetritis and the diffuse or general, septic peritonitis as it is seen in decided septicæmia, ending usually fatally about the eighth or ninth day. And quite another picture is presented by those cases of septicæmia which terminate fatally within one or two days from being infected, without peritonitis or any local affection. But where can we find the boundary? Hitherto, Gentlemen, nowhere. There are inflammations of undoubted septic character, which appear as purely local affections. This has long been known from experience in the great lying-in establishments.

As soon as an epidemic of puerperal septicæmia breaks out in one of them, the number of apparently simple inflammations is increased, even to twice or three times the usual quantity. How can we do otherwise than suppose, from the regularity of the return of these phenomena in all epidemics occurring in such establishments, that a great part of the apparently purely local inflammation is caused by infection?

It is therefore probable that without any actual epidemic, or I would rather say, without any fatal cases of puerperal disease, many cases of perimetritis and diffuse peritonitis depend on infection, particularly if such affections occur in groups. And this is observed from time to time in every lying-in establishment, especially we remark it not unfrequently in our institution here at Halle. It always strikes me as remarkable that from the commencement of the autumn vacation, when the practitioners attending the confinements are less numerous, and are often altogether absent, the cases of perimetritis and parametritis become more rare. This was never more striking than during last winter.

From the middle of July 1870, to January 1871, there were no practitioners attending the hospital, and cases of puerperal inflammation almost entirely disappeared. Finally, we have proof from isolated cases of undoubted septic inflammation, that they have nothing necessarily peculiar in their course, particularly such cases as have a successful termination. Some years ago I experienced the following in private practice: A woman who had borne several children sickened on the sixth day with a severe attack of perimetritis. Even if I had not seen her during the first five days of her lying-in, I could still hardly have doubted the statement that she had been perfectly well during the first five days, as the inflammation set in quite acutely, with shivering, on the sixth day. The peritonitis was not quite limited. The threatening symptoms however were soon mitigated.

On the fourteenth day of the lying-in the illness was at an end, having shown no difference from an ordinary perimetritis. The cause of severe peritonitis setting in so late was at first not at all clear to me, as nothing in the condition of the puerperal woman could be suspected of being the cause. The matter was, however, soon explained, for two days after the woman was taken ill the child also sickened with peritonitis acutissima, of which it died in two days. At the same time, and in the same manner, the child of another lying-in woman sickened and died, the mother herself remaining well.

Further, only a few days after the first-mentioned woman, her sister, having been confined, was taken ill on the second, or rather on the first day after her confinement, with all the symptoms of septic peritonitis universalis, of which she died within a very short space of time. Finally, the same thing happened to a third woman in child-bed. All these five illnesses, of which four terminated fatally, occurred within ten or twelve days. All were attended by the same midwife.

The first of the women was evidently only infected at a late period of her lying-in, and consequently perhaps got over it the easier. Had I never known of the illness of the other women and children, I should, from the symptoms presented, have had no suspicion of septic infection. I have repeatedly seen other similar cases. In this lying-in hospital, for instance, was a woman who was attacked with diffuse peritonitis on the tenth day of the puerperal state, after being perfectly free from fever for nine days. She however recovered. She had had to lie in the same room with several lying-in-women who had been attacked during the first few days of the puerperium with a most severe and deadly form of septicæmia; this was on account of our limited space, which did not allow of any place for the isolation of patients. Here the symptoms were not different from those of genuine diffuse peritonitis.

These examples may suffice to support the already expressed assertion that septic inflammations may present the clinical aspect of genuine inflammations. This may even be the ordinary way in the slighter cases of septic inflammations, and we always probably mistake or overlook the septicæmic affection in isolated cases, while from the observation of puerperal epidemics we can form a conclusive opinion as to the frequency of these slighter cases.

I will here mention that pathological anatomy is no certain guide to us, either in accepting or excluding puerperal septicæmia. For, on the one hand, those conditions are often wanting in undoubted septicæmia which are supposed to be characteristic—lymphothrombosis and gangrenous or diphtheritic endometritis; on the other, the supposition has never been proved that lymphothrombosis never occurs in genuine peritonitis puerperalis; and just as little has it been proved that diphtheritic endometritis does not occur in cases in which there is no question of septicæmia, or indeed of any other severe form of illness. But the same process takes place frequently enough on the puerperal sores of the vulva, without a trace of any general affection, and without the least fever. The so-called diphtheritic coating of these wounds, the gangrenous destruction of the most superficial layers of tissue, is simply owing to the local action of the lochia, and is not necessarily combined with a reaction on the whole organism. Still less certain than in the two anomalies already pointed out, must necessarily be the differences in the other conditions between septic and genuine affections, even if we allow that in septic peritonitis the exudation is generally purulent, and that the parametritic phlegmon is much more considerable.

If we now return to the etiology of puerperal inflammations, from which we started, there still remains that form of peritonitis arising in connection with the bowel. During the last few weeks of pregnancy there is frequent liability to fæcal retention, which, in the puerperal state, at one time gives rise to intestinal irritation only, at another to inflammation of the serous membrane. The colon descendens is just as often the starting-point of these inflammations as the cæcum.

Finally, I cannot pass over in silence the opinion that inflammation may extend from the inner surface of the uterus to the peritoneum through the Fallopian tubes. Respecting this mode of origin, from salpingitis, as it has been called, I will give you my own opinion later on, in speaking of the diagnosis.

We will now turn to the clinical aspect of puerperal inflammations, by which we mean only genuine parametritis and perimetritis, as far as the separation from septic inflammations may be accomplished; and as in the majority of cases they occur side by side, we will consider them together.

In a few words I will make mention of that period of the puerperal state which lies between the confinement and the commencement of the illness, the period of incubation of the disease, as it may be called. This nearly always lasts only a few days, occasionally only some hours, and very rarely it is entirely absent; this is usually observed where inflammation has commenced during parturition, when a tedious labour has been complicated by narrowness of the pelvis.

This early period of the lying-in generally presents nothing abnormal. Premonitory symptoms are mostly wanting, pain and fever in particular. The pulse is usually lessened on the first day, as is the case in a healthy lying-in. The temperature is not raised, except in some cases during the first twelve hours, when it generally shows a transitory and only moderate rise, rarely exceeding 39°.

extended from there to the left posterior, then to the right posterior, and finally to the right anterior corner, so that the uterus was firmly encompassed on all sides by exudation. The absorption of the first inflammatory product had already commenced when the inflammation had completed its circuit of the pelvis. This is the only case which has occurred to me, in which there has been such a long continuance of fever in pure, uncomplicated, puerperal parametritis without supplementary formation of abscess.

But the fever has not always such a regular course, that it rises for one or two days and then gradually and regularly declines until it ceases completely. Cases are not rare in which fluctuations occur, and the high temperature of the second day is again reached once or oftener. In the first few days the morning remission is usually but slight, while, particularly where the complaint lasts a long time, and especially where the affection of the peritoneum has nearly disappeared, the morning remission in the second or third week of the lying-in is usually considerable, or there may be complete intermission. This circumstance easily leads to danger, in that the physician in private practice, who only visits in the morning, may believe that the disease has long since disappeared—an error the more likely to occur, as the woman, on the cessation of the peritoneal affection, feels herself generally subjectively perfectly well, and only complains of weakness. But the continued frequency of the pulse in the morning, even though it may be moderate, 80 to 90 beats in the minute, the bad sleep and appetite, and the continued tenderness on pressure on one or both sides of the uterus, should guard the physician from a deception which, as a rule, is followed by bad consequences.

These are the cases which if not medically observed from the commencement, have apparently quite a different beginning and course to what is usual. Such patients come to us four, six, or even eight weeks after the confinement, and maintain that they have only been ill eight days or a little longer. According to their statement the complaint began with ever-increasing weakness, loss of appetite, and bad sleep, and only in the last few days have there been pains in the abdomen, the hips, or thighs, which are mostly felt when walking in the street. Fever symptoms are denied, and, in truth; at the first examination, the patients hardly exhibit any increase of temperature, only a somewhat greater frequency of the pulse. An examination, however, shows a more or less sensitive exudation in the true pelvis, or in the fossa iliaca. The thermometer shows the considerable rise in the evening temperature.

Do not imagine, Gentlemen, that in such cases, which occur frequently in out-patient practice, the inflammation has begun at a late period only of the puerperal state. In all cases it commenced in the first five days of the puerperium, and the patient, on being questioned, will always admit that between the second and fifth day she had a rigor, and generally abdominal pains for one or two days, which, however, as they soon disappeared, did not prevent her getting up on the ninth day. The rigor is generally looked upon as milk-fever, while in reality it marked the commencement of parametritis, which was either unaccompanied by peritonitis, or only slightly and transiently. For this reason pain was hardly perceived, and up to the fourth week or longer she felt herself tolerably well. Careful medical observation from the beginning would naturally have remarked the fever continuing from the second or third day of the lying-in. In hospital practice these cases only cause trouble, because the women, feeling themselves perfectly well soon after the first attack of fever, can with difficulty be kept in bed.

If we return to the ordinary cases of shorter duration, we must mention with regard to the fever, that though the evening temperature has again become normal, yet in a few cases, about 15 to 20 per cent., relapses occur, which are mostly shorter than the first inflammatory attack, but which occasionally surpass it in duration and violence.

I may mention, though I cannot explain, the rare occurrence of fever with a so-called Typus inversus, that is, with morning exacerbations and evening remissions.

With the rise in the temperature an increased frequency of the pulse is naturally generally associated. But even in this respect a phenomenon peculiar to the puerperal state is not unfrequently observed. Before the inflammation sets in, the pulse, as already mentioned, has usually become slow. This lessened frequency very often continues even on the first day of the fever, so that we have a temperature of 40° C. or more, and with it sometimes a pulse of 50 to 70 beats. Only after the expiration of some days does the frequency of the pulse correspond with the temperature. It however hardly ever exceeds the number of 120 in the course of the disease. The very high pulses of 160 to 180 in the minute, or even 140, belong—at least if continuing more than half the day—almost exclusively to the severe forms of septicæmia, and consequently give a bad prognosis. In this respect the pulse is a more certain guide than the temperature.

Of the remaining febrile symptoms, headache is seldom absent; and although it usually abates with the violence of the fever, it yet forms for many days the patient's chief cause of complaint. Sleeplessness is in accordance with the headache. Profuse sweating usually follows the first attack of fever, and often recurs later. We are not justified in forming a good prognosis from these symptoms. Many fatal cases run their course from beginning to end accompanied by profuse hot sweats.

Of the other symptoms, pain, before all others, should be considered next to the fever. In the majority of cases it sets in at the commencement of the fever, and, as a rule, is most severe at the beginning. Especially in those cases which we call parametritis is it usually so, because the peritoneal affection at first preponderates. Accompanying the pain, and outlasting it, there is marked and often excessive tenderness on pressure. This nearly always appears at first confined to the uterus, and seldom affects the whole of the organ at once, but mostly the lower part of one of the lateral edges, whence it gradually spreads over the whole uterus and its neighbourhood. After one or two days, if rest be maintained, the pain has considerably abated, and in many cases ceases completely. The peritoneal affection has mainly gone, but the parametritis continues, and though the tenderness on pressure may be lessened in intensity, it has, on the other hand, extended farther, often, especially, reaching the other side of the uterus. In the slighter cases this becomes less and less, and disappears sometimes sooner than the fever, and sometimes later.

In cases of pure parametritis, which form the minority of all inflammations, not only is pain wanting, but often all tenderness on pressure, or the latter is so limited, so slight and transient, that without a daily careful examination it may be overlooked. These are the cases in which, with the exception of the first day on which the fever set in, patients feel themselves perfectly well, and in this belief, medical control being wanting, leave their bed too soon, but only to have to keep it longer afterwards.

In many cases in which we are doubtful as to the cause of the fever owing to the very slight local symptoms, this doubt is removed by the appearance of a palpable exudation after eight to fourteen days; in other cases parametritis becomes at least highly probable from the whole course of the affection, an incipient phlegmon of the vulva, and slight tenderness of one margin of the uterus. And, finally, many cases may perhaps be put down to slight parametritis, in which there is absolutely nothing more than fever of some days' duration—cases which repeatedly occur in the puerperal state.

As an extreme rarity, I must mention to you that intense peritonitis may occur without pain or tenderness on pressure, the sensorium being unaffected. I have only witnessed two cases of the kind, and know of no others from description. The first case occurred in a woman, who was with difficulty delivered with the forceps; she began to get feverish a few days after the confinement, without, however, complaining of pain or tenderness during the four weeks she lived, although there were other symptoms of peritonitis, as vomiting, meteorismus, etc., and such a quantity of peritonitic exudation gradually collected, that it could be seen to distend the thin abdominal walls like a sack, according as the position of the body was changed to the right or left. At the post-mortem examination, four weeks after the confinement, there was universal peritonitis and purulent peritoneal exudation weighing several pounds. The inner surface of the uterus was healthy. There was no disease of the vessels. Both lungs were void of air in their lower halves, and there was pneumonic purulent infiltration.

The second case occurred in this hospital. A woman, pregnant for the third time, was delivered by turning, on account of the cross position of the child. After it was born there was excessive flooding ex atonia uteri. On the third day fever commenced, which continued till her death on the twenty-third day of the lying-in.

In this case there was also meteorismus gradually developed, as well as vomiting and diarrhœa. The peritonitic exudation could be easily demonstrated by percussion. There was however no tenderness of the abdomen up to the time of death, although the sensorium remained unaffected. General peritonitis with abundant exudation was found after death; and there was also gangrenous endometritis.

In metritic affections the pain is mostly of a stabbing character, as it usually is in serous inflammations. But the women complain very often of aching pains; sometimes even these after-pains, especially at the commencement of inflammation, constitute their only complaint. But the connection between after-pains and uterine inflammation is this, that the after-pains existing as such may perhaps be only slightly felt by the lying-in woman, but would naturally be very painful with an inflamed womb. For this reason, after-pains which occur under unusual circumstances are always worthy of attention; as in primiparæ, when the labour has not been extremely hurried, but particularly in primiparæ or pluriparæ, when the after-pains only make their first appearance about the third day. These especially denote commencing inflammation. For after-pains depending on partial retentio placentæ generally appear earlier, and are accompanied by a greater loss of blood. Most likely they were such cases that induced Schröder to assert that after-pains occasionally caused fever, whereas it is well known, and has been repeatedly confirmed, that there is no fever with even the strongest after-pains, if unaccompanied by inflammation.

As a direct consequence of the peritoneal affection, vomiting, as well as pain, occasionally, though rarely, occurs, sometimes at a late period, if the peritonitis become diffuse and extend to the neighbourhood of the stomach.

The functions of the intestinal tract are seldom undisturbed: appetite is almost always bad, or fails entirely, according to the severity of the attack; and, as a rule, is only completely restored on the complete cessation of the fever. The tongue is coated, but moist. There is usually constipation, to which there is a particular tendency in the puerperal state. In no small number of cases, diarrhœa with fæculent stools occurs in the course of the inflammation, with more or less rumbling of the bowels, but without pains or tenesmus; this is generally after long continuance of the disease, not at the commencement.

The diarrhœa is not often excessive. The excretion of the urine is seldom interfered with, and hardly ever at the beginning of the illness. If this should happen, it points almost with certainty to implication of the peritoneal covering of the bladder, and this is shown by considerable tenderness in the middle line over the symphysis pubis, even if pressure be so slight that it does not touch the uterus. Ischury and strangury take place somewhat oftener at a later period of the illness, in the second week, and also signify inflammation of the serous coat of the bladder, in consequence of which the vesical walls become infiltrated with serum, and the function of the detrusor urinæ is hampered. The majority of cases of metritis run their course with the symptoms mentioned in five to ten days, the fever and other symptoms declining by degrees. The termination is gradual. More rarely there is a sudden cessation by crisis, as is sometimes observed in other puerperal fevers, unaccompanied by inflammation. In such rare cases of metritis the temperature often falls from a considerable height (41° and more), about 3°, 4°, and even 6° C., and mostly in the course of one night; in the latter case it is less than normal, down to 36·5°, even 36° C. Should the fall be very sudden, and below the normal standard, there will be symptoms of collapse, faintings, oppression of the sensorium, the fear of death, vomiting, and cold sweats. The whole body feels icy cold; the pulse is very small, often slow and irregular. In twelve to twenty-four hours these symptoms have disappeared; the illness is at an end. I have not seen such collapse go on to death in uncomplicated perimetritis.

There are many exceptions to the usually short course described of parametritis and perimetritis. I will first recall those cases mentioned by Schröder, in which general peritonitis appears to commence with high fever. The whole abdomen becomes rapidly tender, as if from a blow, not as in the former cases, the uterus and its immediate neighbourhood only. At the same time there is meteorismus; and generally vomiting. The pains are severe, and the respiration consequently is superficial and quick; the flushed countenance expresses deadly fear. You think you have one of those dreadful cases of general, septic peritonitis acutissima, and in twenty-four to forty-eight hours the patient is again well. All the symptoms disappear as quickly as they come, and after two or three days the fever may have entirely passed away. This favourable event results with or without aperients, with or without opium, with or without ice applications, with every kind of treatment and with no treatment. I look on these as cases of severe "peritonitic irritation," and believe that the first stage of general peritonitis has been actually present, that the peritonitis has been cut short by nature, and as I would remark, without diarrhœa or any other critical symptoms having contributed to this favourable issue. It is only a supposition, but, at any rate, a plausible one, if I assume that these cases depend on infection.

If we turn to those cases which terminate in undoubted pelvic exudation, we find that they agree completely in all particulars with the already depicted type of cases of perimetritis and parametritis in general, only that they usually last longer. In other words, if the fever accompanying puerperal inflammation lasts a few or perhaps only eight days, we comparatively rarely get any exudation that can be diagnosed. But with the longer continuance, the probability of its forming increases more and more. And the longer the fever lasts, the less are the peritonitic pains felt, and they either disappear entirely, or there remains only a circumscribed tenderness on pressure.

These exudations mostly become apparent in the second, or perhaps even for the first time in the third week of childbed. An early internal examination would most likely often discover them sooner; though you know that I have repeatedly warned you against these early examinations, if the object is only to confirm the presence of exudation. As there are almost always lacerations of the vulva or vagina, an accurate exploratio per vaginam at this early period must nearly always be painful, and may sometimes be followed by a recurrence of the inflammation. And besides, the danger of infection is always relatively great at this period, and this must be particularly kept in view in lying-in establishments; and finally, it has hardly any influence on the prognosis, and none at all on the treatment, whether we find exudation a week sooner or later.

Pelvic exudations may at one time be detected by an internal examination alone, at another by an external, and sometimes if large enough they may be felt both externally and internally. Their anatomical position is immediately extra peritoneum, and most frequently between the layers of the lig. lata towards the base, and so bordering on the lateral portions of the roof of the vagina. If the upper part of this ligament only is the seat of the inflammatory product, and if the uterus at the same time keeps a high position, the tumour can hardly be reached per vaginam. It is more rare to find it behind the uterus than at the side, and more seldom still between the uterus and bladder. Next to the lig. latum, the one or other fossa iliaca is perhaps most often the seat of the swelling, and hence it may extend under the peritoneum to the anterior abdominal wall, where it becomes accessible to palpation in the neighbourhood of the lig. Poupartii or higher. In the great majority of cases the seat of the tumour is at the side of the uterus, and is more often perceptible internally than externally. In this situation it generally has a rounded form, but is often only slightly arched towards the vagina, far less than the fibromata and the smaller ovarian tumours; sometimes even the swelling is as flat as a board underneath. At the same time, there are slight inequalities to be felt on the surface, but never such prominent knobs as may be felt in the fibromata of the uterus. The size does not usually exceed that of a large apple. The consistency is firm and solid. In recent exudations the hard swelling is felt surrounded by a softer layer, on account of the still existing infiltration of the soft parts. But after some weeks it has generally attained the consistency of solid fibromata, or it may even be harder. The tenderness, which was but slight originally, diminishes, if suppuration does not set in, more and more with the age of the exudation. It always lies immediately attached to the uterus, and extends not uncommonly outwards as far as the pelvic wall, so that it is stretched out as a bridge between both parts. In the beginning the uterus is generally hampered in its movements by small exudations, but soon, with the larger tumours at least, it becomes so fixed that all mobility is lost. The result of this is that the uterus long maintains an abnormally high position, the vagina is lengthened, its anterior wall is flattened, while the portio vaginalis uteri is shortened. The larger tumours force the uterus proportionally out of the median line towards the opposite side, sometimes only the fundus (lateral-version), sometimes *in toto* (lateral-position). Later, on the contrary, when the exudation contracts, the fundus is often again drawn back to the affected side, whence a permanent lateral-version may arise.

The vaginal roof often acquires an irregular shape owing to the tumour. On the affected side it becomes broader and flatter, and even convex downwards; the latter, however, only when the tumour is deep enough in the pelvis to depress it. If the swelling lies behind the vagina, it usually projects much lower than when in the lateral position, without, however, its upper extremity being within reach. Its form is often less that of a ball, being rather flattened antero-posteriorly; sometimes cord-shaped exudations may be felt, corresponding to the situation of one of the recto-uterine folds of Douglas. Although a moderate degree of displacement of the uterus forwards may take place, it is seldom forced against the symphysis pubis in consequence of the size of the tumour lying behind it, as is so often the case in recto-uterine hæmatocele. The rare ante-uterine exudations, owing to their rounded form, usually force the anterior vaginal wall considerably downwards. Why, with the loose construction and abundant areolar tissue between the uterus and bladder, they so seldom occur, I can only explain by the circumstance that injuries of the soft parts are not common along the whole anterior wall of the genital canal, as well of the ostium-vaginæ as of the cervix uteri; while on the other hand the lateral lacerations of the os uteri and of the vulva, which are hardly ever absent, easily explain the usual occurrence of inflammation in the lateral pelvic regions. An explanation is necessary to show how it happens that inflammations commencing at the side of the cervix uteri almost always extend laterally to the fossa iliaca, less often backwards to the septum recto-vaginale, and hardly ever to the areolar tissue lying beneath the excavatio vesico-uterina.

We gain some information in this respect from the experiments of König. He found that injections of watery fluids or of air into the subperitoneal areolar tissue of the pelvic cavity, did not spread themselves out equally on all sides, but always took certain directions; thus, in injections between the layers of the upper part of the lig. lata, he observed the passage of the fluid into the fossa iliaca first, and then downwards into the true pelvis. Injections made laterally, but deeper downwards and in front of the uterus, passed first of all into the laterally situated pelvic areolar tissue, and to the side of the bladder; then following the course of the lig. uteri rotund. to the lig. Poup., and hence backwards into the fossa iliaca. If the injection was made still further backwards, and on one side of the cervix uteri, it passed first of all into the areolar tissue of that quarter of the pelvis, and then, as in the other cases, into the region above the musc. ilio-psoas. From these results we can understand the great tendency in these exudations to find their way towards the fossa iliaca, whether they travel along the lig. latum to the lateral wall of the true pelvis, or forwards along the lig. rotundum to the inguinal region, and thence to the anterior part of the iliac fossa. In the latter case we generally find the peritoneum covering the floor of the anterior pelvic region on the side affected, lifted up and by degrees pushed back from the lower part of the abdominal wall, so that the exudation, which was originally situated posteriorly, now becomes immediately contiguous to the abdominal muscles, without peritoneum intervening.

Another factor must be taken into consideration with regard to the exudative processes, and their extension in the living subject, on which the experiments of König throw no light. Anatomy, however, does so. I mean the course and direction of the lymphatic current. A large, perhaps the largest, part of the lymphatic vessels travels from the uterus to the fossa iliaca through the lig. lata, in order to reach the vertebral column in company with the art. iliaca ext. This also helps us to understand how inflammations spread into the iliac fossa.

On making an external examination we find that the ordinary exudation of the lig. latum is at the commencement irregularly deposited on the one lateral border of the uterus, which becomes very apparent on comparing it with the other side. Later a rounded tumour becomes developed, which can be clearly defined, although closely attached to the uterus. This tumour lies at a greater or less distance from the abdominal wall, and consequently does not deaden the sound on percussion; but after a long period when it has grown considerably, particularly when suppuration takes place, it extends to the abdominal wall and produces a dull sound on percussion at a comparatively most frequently in the nervus cutaneus fem. ext., and in the cruralis.

The fever, which had latterly been only of an evening, and quite imperceptible to the patient, again becomes stronger. Repeated shiverings, and even tolerably strong rigors show themselves about the eighth or ninth week. If the abscess breaks externally, it is generally in the region above the middle of Poupart's ligament, where pointing, redness of the skin, and fluctuation present themselves. It occasionally, though rarely, breaks in another part, as in the neighbourhood of the incisura isch. major, the perineum, any part of the anterior abdominal wall situated higher up, or the posterior portion of the soft parts at the side of the musculus quadratus lumborum. On account of the thickness of the soft parts in the latter situation, which interferes considerably with spontaneous perforation, it is usually necessary to make an opening with the lancet on fluctuation being felt. In these cases fluctuation is usually felt simultaneously over Poupart's ligament anteriorly, and over the end of the iliac crest posteriorly, and the wave-like motion from front to back may be observed. Before decided fluctuation is observed, we can usually feel the so-called tissue-gaps (Gewebslücken), doughy places beneath the skin, about the size of the top of the finger.

Perforation outwardly, or marked fluctuation, takes place in the majority of cases between the seventieth and eightieth days, but often earlier if the abscess breaks internally. Perforation into the intestine occurs scarcely less frequently than externally, mostly into the lower part of the colon; less often into the bladder and uterus; and especially seldom into the vagina, on account of its thick resistant walls. The most unfortunate, but at the same time the most unusual way that the pus can take is into the peritoneal cavity. The most acute, deadly peritonitis is the certain consequence. But the peculiar property possessed by the peritoneum, in common with all serous membranes of becoming thickened in the proximity of suppurations and inflammations, almost always obviates such an unfortunate event. The putrefaction of the pus in abscesses presents a sad result of another kind. It is rare, and generally, but not always, fatal. It may occur without any complication, and independently of any general affection, in abscesses which have lasted a long time, many weeks or many months after labour. The few cases which I have seen have always been in connection with abscesses in the iliac fossa, which perhaps seem more disposed to become putrid than those of the true pelvis.

It is difficult to say why it should occur in a purely local affection, and so long after labour. But I cannot help suspecting that the close proximity of intestine is to blame, that the gases diffused from the intestine excite a process of decomposition in the previously good and healthy pus.

It is well known that the pus of periproctitic abscesses, even when we are certain that no communication exists with the bowel, often has a penetrating, more or less decidedly fæculent odour, when they are opened. We occasionally perceive the same phenomenon in abscesses of the abdominal wall. Thus, in a patient who had made numerous morphia injections under the skin of the belly with a blunt hypodermic needle, I opened in the course of some months a series of abscesses of the abdominal wall, the matter from which never had a good colour, but looked dirty, and gave out a smell that always poisoned the whole room.

If putrid decomposition sets in in one of the larger pelvic exudations, the woman generally dies either of exhaustion with increase of the fever, or a more or less decidedly marked septicæmia is developed from the putrid mass. The fatal termination is then not far distant, all the symptoms being aggravated, with dry tongue and affected sensorium, slight delirium, and finally coma.

As to whether, in suppuration already set up in the pelvic areolar tissue, provided that the matter is good, the breaking of the abscess is inevitable, or whether even then absorption is still possible, is a question which I must answer in the latter sense. I do not speak here of purulent infiltration of tissue, a condition which in the pelvic areolar tissue of the living subject is not to be diagnosed with certainty, but which may undoubtedly often recover without the formation of an abscess; I mean the absorption of an already formed, though but small abscess.

You yourselves observed one of these rare cases last winter: a woman who had been delivered of twins was taken ill on the second day with pains at the left edge of the uterus. The inflammation, at first limited, soon spread tolerably diffusely over the peritoneum. After the diffuse peritonitis had abated, considerable tenderness remained in the left half of the entrance to the true pelvis, which abated in the third week, but increased again in the fifth. The rest of the abdomen was no longer sensitive. Gradually the psoas muscle became rather hampered in its movements. The leg could no longer be completely extended. In the sixth week a mass of exudation projected above the left ligament. Poupartii, which was felt to be at least 3" to 4" broad from right to left, and more than 2" broad from top to bottom. It felt solid. Towards the end of the seventh week the fever, which had been continuous since the labour, became exacerbated. Repeated shiverings set in, followed by very high temperatures. At the beginning of the ninth week the severity of the fever abated, as well as the tenderness of the exudation, while her general state of health improved correspondingly. At the same time the exudation which lay close to the abdominal wall, showed in its central and most prominent part such decided fluctuation that we thought the abscess would break of itself, or would have to be opened within a week at the outside. However, instead of the fluctuation becoming more marked, it became decidedly less so, and in the eleventh week it only felt indistinctly elastic, and in the twelfth week it had again become quite solid, with, at the same time, some increase in the volume of the tumour. By the end of the twelfth week the fever had entirely ceased. The exudation had at no time been very perceptible per vaginam. She remained in our institution for about five weeks after the return of convalescence. She had become in the highest degree attenuated, but this period sufficed to render her once more blooming and healthy. The tumour was not completely absorbed at the time of her discharge.

There remains still to mention the behaviour of abscesses before and at the time of perforation. Perforations through the abdominal walls and those into the intestine are nearly equally favourable.

In the latter case sudden diarrhœa not unfrequently results owing to the intestinal irritation caused by the purulent discharge, and this symptom should consequently always arouse the physician's attention in cases of long-continued pelvic exudation.

As the flow of pus is mostly very copious, and usually takes place into the rectum, the demonstration of pus in the excreta is generally easy. Hardly more unfavourable is the discharge into the bladder, which is announced by frequent desire to pass water, and some amount of strangury. The abundance of pus at first contained in the urine generally soon becomes less, and there is not often much vesical irritation.

Before proceeding farther I will briefly mention the intraperitoneal, encapsuled abscesses. In comparison with the extraperitoneal they are a great rarity, and we can almost with certainty exclude the intraperitoneal seat of the exudation in nearly every case where it occurs in child-bed, especially if it has been watched from the commencement. The original seat of the tumour at the side of the uterus, between the anterior and posterior pelvic walls, and almost in the middle, as well as its gradual progress towards the fossa iliaca, excludes the possibility of its having an intraperitoneal seat. In addition to this we have the hardness at the commencement, which only gradually, and with increased fever, changes to an elastic consistency, and then to decided fluctuation, while the peritoneal exudation, being fluid from the first, shows the most marked fluctuation immediately after being encapsuled with remission of the fever, or even complete apyrexia. A doubt can only prevail as to the seat of the abscess if we merely get to see the patient at a late period, and if it lies contiguous to the anterior abdominal wall. In this case percussion does not decide. In both cases the extent of the dulness on percussion is considerably less than the extent of perceptible tumour, even though the dulness is usually more complete in intraperitoneal exudations. If the swelling extends downwards as far as Poupart's ligament, its further extension into the lig. latum is perceptible on making a bimanual examination, or, at any rate, is extremely probable. Only in those rare cases, where the exudation lies quite isolated higher up behind the abdominal wall, and does not form a flat tumour, whose edges can be almost grasped from behind, but rather a rounded fluctuating one—in those cases only are we sometimes unable to overcome our doubts as to its seat. If the exudation breaks externally, the probability is always for its being extraperitoneal. Only on its breaking through the navel may we rather think of an intraperitoneal seat.

present time—that is, a year and a half, though latterly the quantity has been less. The patient looks plump and blooming, and has no complaints beyond the purulent discharge and obstinate constipation.

I have already mentioned, Gentlemen, the subperitoneal exudations of the iliac fossa, which not unfrequently occur in the puerperium; but we have not yet thought of the more uncommon inflammations and exudations which establish themselves more deeply on or within the muscular sheath of the psoas, and are known under the name of psoas-abscesses. Still more rare are those abscesses which lie immediately on the bone between it and the iliacus. Fortunately, psoas-abscesses occur very rarely indeed in the puerperal state. They, as well as the ordinary subperitoneal ones known in child-bed, arise through inflammation travelling from the uterus and lig. latum, and not through any primary affection of the muscle, caused by injury at the time of labour, as was formerly believed.

It is often long before the exudations can be felt in these deeper lying abscesses. At one time they lie too deeply beneath the soft parts, at another too far back in the iliac fossa, while at another time again the tension of the abdominal muscles prevents their recognition, if it be not removed by the administration of chloroform so as to produce narcotism. The pains are very frequently less felt in the body than in the hips or thighs, or even in the knee-joint, and the contraction of the psoas, which always takes place, forms a far more constant distinction between them and the superficial abscesses of the iliac fossa, where contraction of the psoas is either absent or is inconsiderable.

The places where the abscess breaks are also different: at one time not above, but some distance below the lig. Poupartii, even in the lower part of the fossa inguinalis; at another time at any part of the iliac crest, from the spina ant. sup. to the posterior extremity of the crest in the lumbar region. Finally, if the matter sink between the muscles, the opening may take place far down on the thigh, even in its lower half.

The time at which such an abscess breaks is usually a much later one. Once I saw an abscess open, after four and a half months' duration, just over the spina ant. sup. The most tedious of my cases recently ran its course; but before that I had seen one where the abscess was opened in the lumbar region after seven months. In this most tedious case which I mentioned, the exudation had been perceptible for months in the anterior part of the right iliac fossa; but it was ten and a half months after the confinement before the still deep-seated abscess could be opened for the first time at the centre of the iliac crest, and fourteen days later more forwards, when it was opened for the second time close above the spina ant. sup. The otherwise strong woman had been reduced to a skeleton. However, in a few weeks she improved almost beyond one's hopes, as happens with most patients of this kind. Only a few of them die, worn-out at length and exhausted by the fever and the loss of appetite; a few die of septicæmia, if, as already mentioned, the exudation becomes putrid, when, not unfrequently, gases are developed in it.

If we now turn from these inflammations of the fossa iliaca to those of the uterus, it remains only for us to speak briefly of the diagnosis and treatment of these affections.

As to the diagnosis of puerperal inflammations in general it is not necessary to say much. In the great majority of cases the simultaneous occurrence of acute fever with pain and tenderness on pressure allows no doubt as to the existence of inflammation, and then the only thing is to define it properly, and, above all things, to determine if it is caused by septic infection or no. Many circumstances require to be considered in order to decide this question—as the character of the previous labour, the injuries that may have been received and their present condition, the possibility and probability of the woman having been infected at or after the labour, her state of health before the occurrence of the inflammation, the extension of the inflammation, etc.

In a certain number of cases of pure parametritis the diagnosis of the inflammation becomes somewhat difficult, on account of all, or nearly all, pain being absent, especially too if we have not watched the case from the commencement, but have first seen it when the slight peritonitic affection has long passed off, and nothing is to be found but the fever, with morning remissions or intermissions. Generally the later exudations give a conclusive explanation; at least if the fever does not pass off quickly.

I must specially make mention of a condition which may easily be mistaken for perimetritis. It sometimes happens that in the first few days after labour a tolerably acute more or less severe fever arises, and that we find great tenderness of the whole of the uterus, and not merely of one border of it; and yet there is no perimetritis. The lochial discharge has ceased. Formerly this was considered to be a sure sign of endometritis, but very unjustly. For the fact was overlooked that, in every acute attack of severe fever, the lochial secretion may be suddenly diminished, or the discharge may even temporarily disappear. And we also not unfrequently observe a cessation, or at least a striking diminution of the lochia at the commencement of acute perimetritis, without there being a thought of any complicating endometritis. But in the above mentioned cases there is another meaning in the cessation, which is usually complete, and lasts for some days. It signifies that there is retention of the lochia, but not suppression of the secretion.

Absorption-fever however arises from the retention, and at the same time the womb becomes again distended and often extremely tender. Thus it is that the symptoms of acute metritis originate; these symptoms, however, quickly disappear, as soon as the lochia have a free exit. This check to the discharge is mostly owing to a flexion—usually anteflexion; occasionally a blood-clot obstructs the cervix uteri. Anteflexion at so early a period of child-bed may be caused by a high degree of laxity of the puerperal uterus, or anteflexion of the pregnant uterus, which, to a certain extent, is always present in connection with pendulous belly, may have been the original cause of the empty uterus becoming bent in a more marked degree.

The majority of such cases were not long ago generally looked upon as cases of endometritis; but the ghost of the so-called catarrhal endometritis vanishes more and more, however little we are able to deny the existence of septic or diphtheritic endometritis as the ordinary anatomical condition in puerperal septicæmia. No single one of the symptoms brought forward as characteristic of this disease can stand the test, neither the cessation of the lochial secretion nor the putrid smell of the discharge, and these were hitherto the best and surest signs. It is at length time to make public the fact that catarrhal endometritis, according to the manner and signification in which it has until recently been received by many, has really no clinical and pathologico-anatomical existence.

Ten years ago or more the complaint seemed destined to play a special rôle in the pathology of the puerperal state. Many cases of peritonitis were thought to have been discovered to be owing to inflammation which had travelled from the inner surface of the womb through the Fallopian tubes, that is, were caused by "Salpingitis."

At first sight this doctrine has certainly something plausible about it, but it is little more than a hypothesis. We no doubt generally find in cases of severe peritonitis considerable swelling of one or both tubes at the fimbriated extremities, which are of a bluish-red colour, and the ovary filled with a quantity of pus. But as the inner half of the tubes never take any part in these inflammatory appearances, we must conclude that the inflammation has spread from the peritonium to the tube, and not the reverse. I do not wish to assert the impossibility of the passage from the inner surface of the uterus through the tubes to the peritoneum; but the proof that this way is frequently taken, fails entirely, as well as, in non-septic peritonitis, the anatomical substratum also—that is, endometritis. We find that, as I have already stated, there is a better clinical and anatomical foundation for it in the wounds of the endometrium, which form the usual starting-point, from which the inflammation is propagated to the peritoneum by means of the submucous and pelvic areolar tissue.

As an inflammatory process is established in every wound, we may certainly speak of this as endometritis. But this is very different to general endometritis, whose existence and passage through the tubes we are not justified in accepting as a frequent pathological process.

We may say the same of oophoritis puerperalis as of endometritis. Great frequency and importance has been attributed to it, while it occurs extremely rarely as an independent disease of any consequence.

Even at the present day the greater number of physicians diagnose oophoritis, if inflammation occurs at the side of the uterus in a lying-in woman. The post-mortem examinations however afford hardly any ground for this diagnosis. Certainly in those dying of puerperal septicæmia we often observe affections of the ovary, consisting either of simple swelling and serous infiltration of the ovary, or of the formation of thromboses in the vessels, or finally of small abscesses. These changes however always exist in connection with general peritonitis, and there is not the slightest foundation for the assumption that the ovary can be otherwise than secondarily and incidentally implicated in the disease. But in simple cases of peritonitis, not depending on septicæmia, we find, should they prove fatal, that the substance of the ovary is always healthy, even if its surface, as of that of the other intestines, is covered with flakes of pus. The slight physiological importance of the organ at the puerperal period, as well as the anatomical condition, do not support the opinion that the ovary can be frequently the starting-point of puerperal inflammations.

The only cases of much importance of ovarian affections occurring in the puerperal state are the extremely rare ovarian abscesses, which may be principally distinguished from the ordinary pelvic abscesses by their perfectly rounded shape, their slow maturation and lateness in breaking. In fact, it seems that more than a year sometimes passes before these abscesses break.

If we now pass on to the treatment, we must think of prophylaxis in the first place. This will be essentially limited to guarding against infection, and that which may give opportunity for it—injuries or wounds. It is well known that our power is very restricted in this respect. As to infection, which we may be the direct cause of, the greatest caution is necessary in order to avoid it with regard to our hands, and to all instruments used, and above all things with regard to the dirty linen in lying-in institutions; sponges should be removed, and many other things which I will not discuss to-day. I will only once more remind you that internal examinations in the first period of child-bed involve decided danger, as well as injections into the vagina, if, especially as in institutions, they are frequently employed. For then the nurses and attendants become inevitably more careless with regard to the urgent necessity for strict cleanliness, and herein lies great danger. Still more strongly must I protest against injections into the cavity of the uterus during the puerperal state, even when performed by the physician himself, at least when they are used indiscriminately as soon as the lochia show signs of a putrid condition. More often than otherwise harm is done by such meddlesome officiousness. These injections should be reserved for the rarest cases, more especially where, owing to retention of coagula in utero, extensive decomposition and real danger may arise.

Of the remedies when inflammation has already commenced, local blood-lettings are naturally the first to be thought of. But in these cases they have not acquired the confidence of physicians in anything like the same degree as in other kinds of inflammation; there are even many experienced obstetricians who never employ them at all in cases of puerperal peritonitis, because they find them so ineffectual. I am not of this opinion, Gentlemen—at least not in all cases of inflammation. Blood-lettings have certainly no object in those cases of peritonitis in which the septic nature of the complaint has been established from the beginning. They lessen the pain for a few hours, without exerting any other influence on the course of the disease.

And blood-letting is very properly not had recourse to in those cases of peritonitis which are diffuse from the commencement. If, however, we have to do with a really acute but limited inflammation, with decided implication of the peritoneum, local bleeding is then often sufficient to obtain rapid and permanent relief from pain; but the bleeding must be used at the very first, and should not be too scanty. Do not commence with less than ten leeches. The constitution of the woman must naturally be taken into consideration in deciding this question.

Better results are attained by the local abstraction of blood from the port. vaginalis than from the surface of the abdomen. It may appear remarkable that this kind of blood-letting has hitherto been only occasionally employed in cases of puerperal metritis, while everyone knows that much more certain and permanent results are obtained in this affection by the application of two to four leeches to the port. vaginalis, than by three or four times the number to the abdomen.

The puerperal state of the genital organs is certainly not favourable to the introduction of the speculum, on account of the lacerations existing there; but it does not offer any essential obstacle; as little as the lochial secretion prevents the leeches biting. The great vascularity of the port. vag. is more often the cause of the creatures biting immediately, and filling themselves within ten minutes. Caution is however needed with regard to the still patulous cervix uteri, which would easily allow one of them to creep into the cavum uteri. This event, which might very easily happen in a case of retroflexio uteri, on account of the gaping lips of the cervix, as well as in the puerperal condition of the organ, is always followed by severe uterine colic, and generally by a complete failure in the success of the blood-letting. You may with certainty avoid this, however, by previously applying to the cervix uteri tampons composed of small plugs of wadding, several of which should be carried sufficiently deep into the cervix (or you may adopt the means used by Weber in Lemberg, who draws a thread through the tail of each leech, in order to secure them). These blood-lettings have the great advantage that they do not cost the patients much blood (for, at the outside, four leeches are used), and I have repeatedly seen them followed by immediate relief of all the symptoms, and by a short duration of the complaint, so that I strongly recommend them to you.

Of other local remedies, I usually apply to the abdomen fomentations of water of the temperature of the body. Over the otherwise unpleasant poultice they have the advantage of greater simplicity, as it is only necessary to change them every few hours, because the compresses retain their moisture for such a length of time, and the temperature at any rate remains constant, which is not the case with poultices, especially if there be insufficient attention. But above all, they appear to allay pain in the same measure as poultices, from which we may conclude that they have an equally favourable effect. That warm poultices may favour the undesirable change of exudation into suppuration is indeed only a hypothesis, but it is one that has a great show of probability. Where there is already suppuration, poultices are naturally applied in order to draw the abscess to a head.

I use applications of ice in cases of decidedly traumatic peritonitis—generally they are such cases as arise quickly after a severe labour—and continue their application, according to circumstances, for about eight days. I have almost entirely given up the use of this remedy in other kinds of puerperal peritonitis. There is this disagreeable in connection with ice applications, that at first they promote the peristaltic action of the intestines, until they become accustomed to the irritation of the cold. Vesical catarrh, too, not seldom occurs—at least as the result of its being applied for several days.

As to the use of internal remedies, the first question is whether the bowels had been opened at the commencement of the metritis, and whether they have been lately opened or not. If not, this should be provided for by an enema, or by a dose of castor oil. It is better if it is not needed, and for this reason among others, it is best not to follow the custom of only having the bowels acted upon on the fourth day of lying-in; it is altogether better to provide for it earlier. If diffuse peritonitis sets in very severely from the first, I give no aperient at the commencement, but at once pass to the opium treatment. I give opium in doses of five centigrammes four or six times, or even oftener, in the twenty-four hours, or the corresponding dose of morphium muriaticum endermatically, and in diffuse inflammations allow even six or eight days to pass without troubling about the action of the bowels. Sometimes even then we see a return of the inflammation follow the first defæcation. In recent cases of diffuse peritonitis, absolute rest of the bowels must be our principal object.

In the great majority of cases of puerperal perimetritis and parametritis however, when the affections are limited, I hold myself passive, as far as internal medicaments are concerned, and give a neutral salt, or some other indifferent remedy of that kind. If diarrhœa complicate the case, I do not usually check it at first; but if it continue, treat it with alum, opium, gum, etc., when it generally quickly subsides.

It is not necessary to tell you that strict low diet (water-gruel) is necessary at the commencement of every para- and perimetritis. But in those frequently tedious cases of parametritis, particularly such as go on to the formation of abscess, we are often obliged, even though the fever continues, to adopt a nourishing, easily-digestible diet, if we do not wish to reduce the patient too much.

In the beginning of peritonitis the necessity for maintaining absolute bodily rest is self-evident. But on the subsidence of the inflammation, it must most strictly be impressed upon the patient that any violent movement may rekindle the peritonitis. Even where all peritonitic symptoms have long since disappeared, and the process continues, either certainly or merely probably, in the pelvic areolar tissue only, the bed should not be left till there has been no fever whatever for several days. Women who are not in clinical establishments, and who are not under medical supervision, and even many who, though under supervision, are not examined thermometrically of an evening to judge of the absence of fever, very generally deviate from this rule, because they feel themselves almost well after the first peritonitic seizure is over. But relapses are the ordinary consequence of this error, and in all probability this early getting about whilst inflammation still exists, is exceedingly favourable to suppuration taking place in the exudation. In hospital practice abscess-formation is very rare, on account of the strictness with which the lying-in patients are kept confined to bed. In about 250 cases of peri- and parametritic affections occurring among those who were delivered in this hospital, there were only three cases of the formation of abscess.

Next to the want of proper bodily rest, relapses—either on the fever abating, or during the convalescence—are principally occasioned by errors of diet and constipation. Where parametric exudations exist, it is particularly necessary during the fever of many weeks' duration, and even after its disappearance, to attend carefully to the regular evacuation of the bowels at least every two days; and this may eventually be promoted by mild remedies, especially enemata.

With respect to the treatment of exudations remaining after all inflammation has gone, I will only remark that iodine has no effect in these cases, whether used externally or internally, although it is the remedy most generally employed, but rather that strengthening of the body and good nourishment are really required. We thus best favour absorption of the exudations by the more active elementary changes. Warm baths may also be advantageous at an advanced period of reconvalescence, and may promote absorption. It is very doubtful indeed whether mineral waters containing iodine and bromine do so to a greater extent than the ordinary warm-water baths. As such exudations disappear with time alone, and in tolerably recent cases often astonishingly quickly, we may be quite certain that many medicinal springs owe their celebrated name and repute to this influence.

It would take too long to-day to speak of the treatment of suppurating exudations.

ON EXCISION OF JOINTS.

BY

RICHARD VOLKMANN.

ON EXCISION OF THE LOWER EXTREMITY.

If I dilate to-day upon excision of joints, and take it as one subject of these lectures, I am merely acting upon request made to me on many sides. I feel it my duty to accede to it, as great experience in the treatment of diseases of the organs of locomotion has given me full material for observation, and has placed me in the position to act independently in such operations. It would not be possible to treat upon the excision of the six large joints of the body in one lecture without going beyond the limited space allotted to me. The reader must, therefore, excuse me if to-day I merely speak on excision of the lower extremity.

I have endeavoured to produce something really practical for the medical man who wishes to practice surgery, and, if necessary, also to excise a joint. The indications, therefore, for this operation have been as well detailed and described as my own observations and the present state of the question of excision can show. I have described the practical part of the operations and their after treatment so minutely, that I am in hopes that the surgeon may be enabled, after having studied this paper, to have gained a clear insight into the same. The manuals on operative surgery do not, in my opinion, always come up to the mark. The technical part is often only understood by those who have practical experience of the same; the after treatment and the different possible mishaps are scarcely touched upon. Instead of this, a legion of names and different modes of operating are mentioned, which are utterly useless for any but those who make a study of the history of surgery, and are handed down from generation to generation with a reverence otherwise not in harmony with the present age.

These points should be excluded from the programme of clinical lectures. I have made full use of my right, granted to authors, to give the widest space to personal observations and personal experience. To give a verdict respecting the value of excision of the joints, and the limits to which it should be restricted, is not possible at present. Future statistics are needed to decide this.

EXCISION OF THE HIP-JOINT.

Excision of the hip-joint has to be considered in scarcely any other cases than those of caries or gunshot wounds.

With regard to caries I only have recourse to excision if I presume that without this operation death would ensue. Nevertheless, I cannot consider the prognosis of a coxitis in the stage of suppuration in as unfavourable a light as many surgeons do in the present day. I have, after all, seen many cases recover without having required excision of the joint. I consider that in cases of chronic coxitis in children, followed by suppuration, even after the joint has been opened, recovery is the more frequent issue, although many surgeons may be of the reverse opinion. Each succeeding year gives me many new proofs of this fact. I therefore am at a loss to understand how it is that surgeons can excise a hip-joint in children except as a last resource—nay, that, in fact, many recommend excising the joint by all means as soon as abscesses have formed and the patient is attacked with feverish symptoms. In a far different way we have to deal with those cases that occur in adults. Here I have myself seen the greater majority succumb under a conservative treatment as soon as suppuration has taken place. The suppurative coxitis, however, is rare in grown-up persons, and is mostly combined with tubercles of the lungs. In the following remarks I shall only consider the coxitis which takes place in children.

Nobody, I dare say, will attempt operating in those cases, and these are decidedly the more frequent ones, in which abscesses form with little or no fever; at the same time the nutrition of the patient remaining satisfactory. The joint is at that time not unfrequently almost entirely obliterated, and filled with granulations suppurating scantily or not at all, and yet the examination may show at times considerable defects in the bone, and displacement of the trochanter. It may even happen that at the commencement of the suppuration the remaining portion of the head of the femur is already entirely united with the acetabulum by osseous connection. The real disease of the joint therefore has run its course. An excision undertaken at that time cannot be of any use if a profuse and debilitating suppuration should take place from the fistulæ. I have collected several specimens in which a complete osseous anchylosis of the hip-joint has taken place, and nevertheless the patient succumbed on account of the profuse suppuration, and at the post-mortem examinations it was proved that the joint disease had run its course, and nowhere an affection of the bone remained. The patients in these cases died in a similar way to those whose femur was fractured by a shot, and where, nevertheless, consolidation of the bone had already taken place; and likewise you may notice that children, who have been suffering from spondylitis, ultimately succumb owing to fistulæ of a psoas abscess, although the caries of the vertebræ may have entirely healed up, and a firm synostosis of the originally diseased vertebræ has taken place.

I believe it is incumbent upon me to draw your attention particularly to these facts, as I myself have been once almost on the point of excising a hip-joint in a similar case. Excision, therefore, is not permissible if the joint should prove absolutely immoveable, even with employment of some force, after the patient has been put under the influence of chloroform, although the suppuration may be rather a profuse one. In case of necessity an incision merely exploratory will give a clue. In other cases the favourable issue of a fistulous coxitis may depend upon the simple fact that the abscesses are merely periarticular ones, the joint itself being only slightly diseased. It is true that those abscesses which form around the joint are by far less frequent at the hip than at the knee; yet I have seen a sufficient number of cases to prove the above-mentioned fact.

On the other hand, you certainly cannot commit a greater error than by delaying too long excision in severe cases, and by operating only then when the patients have lost flesh, and are debilitated to a great extent. In those cases the issue will almost always be a fatal one. And yet I should not like to say, when you are called to a case at such a late period, that by all means you should not excise. The excision may still be the means of subduing the pain immensely, and occasionally you may yet save a life that otherwise decidedly would have been lost.

Excision, therefore, ought to be performed when, in cases of chronic coxitis with formation of abscesses and fistulous openings, the suppuration becomes abundant and when with increase of the fever at night-time the children begin to get weaker and weaker. And likewise excision ought to be undertaken when in a case, originally a chronic one, and without any suppuration, the dry granulations which have filled the acetabulum, and, almost without any symptom, did their mischief, suddenly give way, and, with high increase of temperature, an acute suppurating coxitis sets in. Excision ought to be performed at once when an iliac abscess which is forming shows that the pus has perforated the acetabulum, and found its way into the pelvic cavity; and, lastly, excision ought to be performed when during the suppuration the head of the femur has in reality left the acetabulum, and is lying dislocated beneath the muscles.

Allow me now to make some further remarks about these four points just mentioned.

In the first case, where the disease is a very chronic one and in connection with increase of temperature at night, with profuse suppurations, and with a decided breaking down of the whole nutrition of the patient, you will be sometimes able to omit the excision and to replace this operation by scraping out the fistulæ, and even the joint itself, with half-blunt spoons. Whether this will be practicable can in most cases only be decided on the operating table, and after the child has been put under chloroform. Abscesses which have not yet been opened have then to be opened, and these, as well as also the fistulæ, to be scraped out with round or very small boat-shaped spoons, following up with slightly corkscrew movements the course of the fistulæ down to their original depth. Large quantities of lardaceous, yellowish-looking granulations, which impeded the healing, are thus got rid of. There is no danger whatever of making false passages, or of penetrating into the sound tissues, nor of even removing these, as, quite independently of the indurated condition of the tissue in the immediate neighbourhood of the fistulæ and abscesses, it is impossible to injure with these instruments tissues of normal texture.

The scraping out is followed often by the most favourable result, which shows itself by the immediate lowering of the temperature, by the contraction of the fistulæ, and by the probably ultimate healing of the same. Sometimes one is enabled to penetrate with this kind of instrument down into the joint, and to spoon it out thoroughly, to scrape the head and neck of the femur as well as the acetabulum, and to remove various parts, sometimes even to extricate loose pieces of the head. This can be done without much difficulty in those cases, although they are rare, where the fistulous apertures have opened just in front of the femur, under Poupart's ligament.

The ultimate result in these cases can often not much differ from that of a real excision, although the injury caused by the operation is of a much milder effect, and may even not be followed by feverish symptoms. This procedure of spooning the bad granulations out of fistulæ and abscesses, which has lately been more minutely described in a small pamphlet by the first assistant of my clinique, Dr. M. Schede,* is applied by us in all chronic suppurations of joints and bones, where we think it possible that excision might be dispensed with. This has to be repeated, in order to produce a decided cure, every few weeks, or at longer intervals. As soon as the healing process comes to a standstill, the granulations begin to get pale and flabby, and the suppuration is again increasing. These measures, however, have often a marvellous effect, principally when in connection with a regular drainage system. At the hip-joint it may happen that the spoon enters now and then into the substance of the great trochanter, revealing a central cavity with a sequestrum, yet the hip-joint is merely slightly diseased. I have lately operated upon two such cases, and in one of them have excavated the whole neck of the femur, instead of having recourse to the excision of the joint. The patient, a girl aged eleven (Anna Kuntze, of Polleben), who showed the usual symptoms of a suppurating coxitis, with great shortening of the limb, adduction of the femur, but with strong eversion of the same, and several fistulæ suppurating freely in the neighbourhood of the hip-joint, died nevertheless afterwards of albuminuria. The cavity in the great trochanter and in the neck of the femur, however, had meanwhile shrunk to the size of a bean. The entirely dislocated head of the femur stood freely moveable, slightly externally from the anterior inferior spine of the iliac bone (viz. Luxatio supracondyloidea). Its cartilage was at all points in its naturally polished condition and well preserved; the capsule merely distended, not perforated, of normal appearance; the acetabulum partly filled up; the affection of the joint itself, which had produced the dislocation, entirely healed; nowhere a drop of pus. Excision of the head would have been utterly senseless in this case.

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* Dr. M. Schede, "On the Use of the Sharp Spoon in the Treatment of Ulcers." Halle, 1872.

With regard to the second case—viz. the acute or subacute exacerbation of a coxitis, which hitherto has taken a favourable course, in so far as it has shown very slight or no disturbances in the general health and nutrition of the patient—we must here mention, as already above alluded to, that the disease known under the name of Caries sicca occurs very frequently and in a highly developed manner in the hip-joint. Dry and altogether few granulations, which take their origin from the periosteum of the neck of the femur, as well as from the spot where the synovial membrane is reflected, begin to grow by eating their way into the bone, and destroy neck and head of the femur, as well as likewise the acetabulum. Very often nothing is left attached to the great trochanter but an excavated nodulated fragment of the neck of the femur, the rest of the neck and head of the same having disappeared entirely.

In another case a small fragment of the head is lying in the acetabulum, filling it up more or less, but united to it by granulation or cicatricial tissue. Although the process of destruction has been going on to such a degree, the pain nevertheless may be but trifling. The joint is fixed and motionless at an early period; the surrounding parts are not swollen; more frequently even regular atrophy has taken place in the soft parts, and then the bony protuberances are well marked. The children walk about limping. They have perhaps never been laid up during their whole illness, and, if they should belong to the lower classes, may have even walked long distances to school, although the process of destruction has continually been going on. If they are examined now on account of the increased shortening and distortion of the limb, it is soon apparent that the entire hip-joint has been destroyed, and serious osseous defects have taken place, although in the exquisite cases no suppuration and opening of abscesses have occurred.

In some cases, however, a fistula has formed at last, but the joint is almost entirely obliterated. We have merely a very limited suppuration of the joint before us; a small quantity of pus has formed, and the general state of health is even now not at all disturbed, or only to a slight degree.

These are the cases where sometimes quite suddenly excision may become absolutely necessary, when, in consequence of an over-exertion, or a severe cold (?), or from some unknown reason, the granulations in the destroyed joint are transformed into pus, and, with severe symptoms, a complete suppuration of the hip-joint, or even a destruction of the same by fetid discharge, takes place. This may happen so suddenly that time is scarcely left to excise the joint. It has happened twice to me that under those circumstances I had to perform excision on patients who had not been laid up before, and yet some few days after a sudden alteration in the aspect of the disease and the commencement of the suppuration sufficed, that of the neck and head of the femur merely some few fragments remained unabsorbed.

In another similar case, which, however, was not of so rapid a course, a large accumulation of foul air had taken place in the subgluteal abscess lately formed, and, being in open communication with the hip-joint, escaped with a rushing noise as soon as the abscess was opened. In such cases it is quite clear excision only can be of avail, and principally as it does not happen so very rarely, that either the head separated through the suppurating process, or at least larger pieces of the same may be found as loose sequestra in the acetabulum. In the very acute cases, however, excision will seldom be crowned with success; I, at least, have not been successful. At other times the suppurating process in the hitherto dry (by caries sicca) destroyed joint is not so acute; and here I should like to recommend operation for excision at the earliest possible period.

Excision therefore has to be performed as soon as a coxitis which up to that time ran its course without any fever, and under the just described symptoms of caries sicca, is followed up by a serious suppuration of the joint. A characteristic symptom for these cases is the suddenly occurring mobility of the joint which originally had been anchylosed. In most cases it is possible, when the patient is under the influence of chloroform, that by moving the joint, friction can be clearly felt.

As soon as we are able to diagnose, and principally by the more sudden appearance of an iliac abscess, that pus has found its way into the pelvic cavity by perforating the acetabulum, we must excise the joint immediately. The prospects of ultimate recovery, however, are in these cases very slight, which is also shown at the post-mortem examinations, where we mostly find an intrapelvic suppuration, a laying bare of the bone, and carious destructions on the inner surface of the pubic, ischiatic, and iliac bones.

The opinion of the French surgeons, that we ought not to operate as soon as the acetabulum is much diseased, might rather, therefore, be reversed, as the excision of the joint gives us the only possibility of getting quickly rid of the suppuration, and opens the way to a possible healing of the abscess. The perforation of the acetabulum may take place in three different ways—either the head of the femur may produce by its pressure a regular and, by degrees, increasing hole in the acetabulum, which had been thinned through atrophy to the mere thickness of paper; or similar, as in necrosis, small, round cloacæ, real osseous fistulæ may form; or, lastly, suppuration may set in in the Y-shaped cartilaginous interstice. The separation in the last-named case may be so well marked that it may be diagnosed already by the passive mobility before the autopsy—a circumstance which I could verify in two different cases. After removal of the head of the femur, it may occasionally be necessary to widen the perforation of the acetabulum with a gouge or a sharp spoon.

I can relate an almost incredible success of excision in the following case. It is that of a boy, ten years of age, who suffered from caries of the right hip-joint. Although an abscess had formed at the outside of the trochanter, under the fascia lata, which was opened, still the general health was remarkably good, and we hoped for cure without excision. High fever, however, set in, the fistula at the trochanter suppurated greatly, the abdomen over Poupart's ligament became remarkably tender upon pressure, a deeply-seated hard swelling was here to be felt with elevation of the iliac muscle, and the iliac bone appeared thickened. Perforation of the acetabulum was now evident, and excision was resorted to. On examination of the acetabulum, commencement of suppurative destruction of the cartilaginous interstices was found, and exactly at the spot where the interstices unite, a portion of the iliac bone of the size of a finger-nail was necrotic through its entire thickness. The necrotic piece was removed by the elevator, letting loose an enormous quantity of thick yellow matter out of the pelvic cavity. This operation was followed by immediate decrease of the feverish symptoms, and after about four months the patient was discharged with an excellent moveable hip-joint, shortened only by half an inch, and able to walk without the assistance of a stick.

A real dislocation of the head of the femur with suppurating coxitis, and where the head is enveloped with pus, decidedly demands excision. For, independently of the circumstance that the operation will be greatly facilitated through the dislocation, and very often merely consists in the opening the abscess and sawing off of the head, the operation therefore being only a slight injury to the patient, the excision promises in these cases, after the healing, a far better result with regard to the functions of the limb than in those cases where no operative measures have been undertaken. The operation removes the bad position of the generally strongly bent and adducted femur, as well as also the well-marked increased turning of the point of the foot—a circumstance which always occurs in the cases of spontaneous cure. The young surgeon, however, must not forget that, independently of the displacement of the head of the femur in the acetabulum, which has been enlarged by caries, or the displacement upwards of the femur after the head has been entirely destroyed, or, what is similar, the separation of the head by suppuration in its interstice from its neck, all circumstances which in the living subject may more or less have the appearance of a real dislocation, and very often cannot be diagnosed with absolute certainty,—the young surgeon, I say, must independently hereof not forget that the most typical cases of spontaneous dislocation of the femur occur in those inflammations of the hip-joint which do not bear a destructive character, and do not require excision. Acute, serous, or also purulent effusions, which nevertheless, however, get quickly absorbed, push the head, which may be slightly or not at all diseased, out of the socket, and as soon as the dislocation has taken place all the inflammatory symptoms quickly subside. This occurs principally in cases of typhoid fever, now and then also in exanthematous diseases, in cases of rheumatic fever, and even after injuries to the joints. In many cases the ætiology remains quite obscure. These are the spontaneous dislocations which permit the trial of a reduction, just as in cases of traumatic dislocations. I have succeeded with reduction in cases of no inconsiderable number, and the joint regained entirely, or almost entirely, its former movements. Occasionally, however, after having succeeded with the reduction, the joint nevertheless opens, and necessitates its excision after all.

With regard to the question of excision, only those spontaneous dislocations are with difficulty to be decided upon, which accompany the acute diffuse osteomyelitis of the upper part of the femur. The suppuration may here have extended from the diaphysis to the joint, the head of the bone may be more or less destroyed; in such a case the end is almost certainly fatal. But the suppuration in the bone may merely have extended into the great trochanter, and perhaps even into a part of the neck of the femur, producing only, on account of its immediate neighbourhood to the hip-joint, an acute exudative inflammation or catarrhal suppuration, which effects a dislocation of the head, but after a while retrogrades. In all joints we must well distinguish between these two kinds of osteomyelitic inflammation of the joints; the slight one; and the serious, destructive one; nowhere, however, is it more difficult to recognise occasionally the real state of affairs than in the hip-joint, when it is certain that the same is attacked with inflammation and suppurating fistulæ penetrating down to the immediate neighbourhood of the joint. How difficult it may sometimes be to form a correct diagnosis the following curious case may fairly illustrate. The case was that of a girl, aged ten and a half years, who showed all the appearances of a severe coxitis in connection with serious destruction of osseous substance. The limb was very much shortened, much adducted, and at an angle of 90 degrees rotated inwardly. At the upper part of the femur severe swelling and many fistulous openings, freely discharging. The hip-joint almost absolutely immoveable. The point of the trochanter drawn by 2 inches backwards and above Nélaton's line. The latter circumstance, as is well known, is considered as a proof that we have either to do with a dislocation, or at least with an equivalent displacement of the femur backwards and upwards on account of a destruction of the hip-joint; and we also considered it in this sense, though the previous history of the case warned us to be on our guard; an acute osteomyelitis was originally the commencement of the disease, and by probing we found in the middle of the trochanter a cavity containing several sequestra. We therefore came to the decision to lay the trochanter bare, open it with a chisel, and to extract the sequestra and to let it entirely depend upon the issue of the operation, whether we should have recourse to excision or not. The laying open of the great trochanter, however, exposed to our view a cavity entirely closed up towards the neck and joint, and surrounded by a healthy ivory-like osseous tissue; we extracted the sequestra and gave up the idea of excising the joint. The operation had no effect whatever, either upon the local symptoms or upon the disturbed general health of the patient. The cavity of the bone which we had laid open got smaller by degrees, and healthy granulations sprang up; the other fistulæ, however, began to secrete more pus. The patient got weaker, and again we had to consider the advisability of an excision. Several examinations under the influence of chloroform had proved that there did exist a slight mobility, the movements were easy, no crepitation. Handling, however, the slightly œdematous femur more carefully, there was no doubt left that the whole upper half of the same was somewhat thickened. According to all probability some minute sequestra were here to be found, or closed osteomyelitic abscesses. Probing gave us no clue, nor the dilatation of different fistulæ. We could not make up our minds, but at last we were convinced that it was next to impossible to decide whether the pus was principally secreted from the joint or from the upper part of the diaphysis of the femur; we therefore declined to excise the joint, although we had not the least doubt about the suppuration of the hip-joint; the child soon afterwards died. At the autopsy we found the joint in a healthy condition and the head in the acetabulum. The last remnants of a diffused osteomyelitis, however, could be traced along the femur down to the knee-joint, although the parts around the trochanter had been mostly implicated. The bone was everywhere thickened, in a sclerotic condition; fistulæ were to be seen in different parts; they were covered with a thick pyogenic membrane and were mostly traced in the longitudinal section of the bone. Here and there some few sequestra were discovered which certainly could never have been found by any operation; not the least trace of a medullary canal.

The most curious circumstance, however, was the following one, and this also gave us a clue to most of the difficulties in the diagnosis mentioned above. The femur, which was now in a thoroughly sclerotic condition, evidently had been at an earlier period in a very soft state (in a state of osteomalacia). The neck, the parts around the trochanter as well as the upper parts of the femur, were perfectly bent to the shape of an S; the point of insertion of the round ligament, therefore, which in the normal state is situated almost in equal height with the point of the trochanter, had been removed 2⅓-inches beneath this spot. The inward rotation of the leg (about 90 degrees) had not been accomplished in the hip-joint, but in the bone. In the normal position of the epiphysis of the knee-joint the caput femoris was pointing almost straight forward instead of sideways. The cartilage of the hip-joint was entirely intact, and not one drop of pus to be noticed in the joint, but the slightly thickened capsule was firmly attached to the much shortened neck and even to the head of the femur. The inflammation of the shaft of the femur therefore had at one time, if also in the slightest form, crept on to the hip-joint and had here produced, as it happens in similar cases, a slight adhesive or obliterating inflammation of the joint, which resulted in fixture and anchylosis of the joint. It was impossible to make a diagnosis; amputation at the large trochanter might perhaps have saved the child·

Mr. H. Leisrink, assistant-surgeon to the hospital at Hamburg, has quite lately published a minute report about the statistical part of the excisions of the hip-joint. According to his statement, the mortality after excision on account of caries is 63·6 per cent., deducting all uncertain cases as well as those which had not been followed up to the healing of the fistulæ. This is sufficient reason to recommend the operation in those cases where the disease is likely to end fatally without it, but it shows likewise the necessity of weighing well the pros and cons of each especial case, and above all of throwing one's whole energy and care into the treatment of coxitis in its incipient stage.

On gunshot wounds of the hip-joint we possess two new and really classical works by the American military surgeon, George Otis. They treat on the minutest details up to the time of the publication, the whole statistics of excision and exarticulation of the hip-joint, and prove that after either operation it is quite exceptional for a patient to recover, but show likewise that a pure conservative treatment has no better result, in fact slightly worse than after the operations. The patients with gunshot fractures of the hip-joint nearly all succumb. The same experience holds ground likewise, I am sorry to say, with us in reference to the late Prussian-Austrian and Franco-German wars. I myself saw only about fifteen cases of this injury. Three times I resorted to excision, twice after the battles of Beaumont and Sedan, once in Dijon. All three patients died; two, as I believe, principally in consequence of bedsores; in the third case we found during the operation a large splintering of the pelvic bone. Twelve cases which were treated on the conservative principle succumbed likewise. Amongst the latter there was one in whom the head of the femur was extracted by an incision into the joint, and was found separated and divided into two parts by the shot. Now and then I chanced to see a patient who had undoubtedly received an intracapsular gunshot fracture of the neck of the femur, which, however, was not diagnosed, and who, after exfoliation of some fragments of bone, recovered with a slight shortening and anchylosis of the joint.

If we now leave out of our reckoning those cases which are beyond any possibility of help on account of simultaneous injury to the abdominal viscera, or on account of severe fractures of the pelvic bones, and examine anatomical preparations of gunshot wounds of the hip-joint, of which I possess a small but very instructive collection, we must soon be convinced that for many cases an operation must decidedly improve the chances of recovery. Whether the chances may be much more favourable for the patient to recover from such an awfully severe injury is, however, another question. For very often the whole head of the femur, or large fragments of the same, are lying perfectly separated in the cavity of the joint, and recovery therefore can only be attained by the extraction of these parts. If we cut down into the joint for this purpose it is more than probable that we likewise shall remove the sharp points from the fragments of the diaphysis or even the trochanter, which perhaps has been entirely crushed, and thus excision is performed. And not very rarely the ball may be imbedded in the neck or head of the femur, or it is lying in the acetabulum amongst the osseous fragments, and therefore can only be removed by either opening the joint or performing excision.

Although the results up till now have been unfavourable, it will be our bounden duty to cultivate in any future war as much as possible excision of the hip-joint. According to my opinion most of the patients who had to submit to excision of the hip-joint during the last war succumbed, because the operation was either performed too late or because it was impossible to bestow upon the sufferers that care which is necessary after such an operation. The two points therefore ought particularly to be considered. As long as we are not able to put these patients on good beds in order to place the limb in proper position, we dare not expect success.

We still have to mention in some few words those cases in which large angled anchyloses of the hip-joint, which do not yield to the brisement forcé, and which condemn the patient to the use of the crutch, as he is unable to touch the ground with his foot, make an operation desirable. This operation, it is true, cannot be called a real excision, but it consists, according to

Rhea Barton's plan, in sawing through the neck of the femur, with or without excising simultaneously, a wedge-shaped piece of bone in order to facilitate the reduction. This case therefore is one of an Osteotomia simplex or cuneiformis. The operation altogether may have been performed perhaps twenty times with a mortality of 50 per cent. If we consider that this operation is merely one undertaken—*par complaisance*—in individuals otherwise healthy, I think we must hesitate before having recourse to it. Fortunately, however, it will be in most cases of young persons, where growth of the bone has not ceased, and particularly in younger children, possible to substitute for the operation breaking of the neck of the femur, or which is still more preferable, fracturing the bone just below the point of the trochanter. I have succeeded several times in this operation. Quite lately I achieved a most brilliant success in a boy, aged seven, who was suffering from an old dislocation of the head of the femur on the dorsum ilei after coxitis, combined with a contraction of the hip at a right angle, and in whom I was fortunate enough to fracture the femur at the great trochanter. After consolidation of the bone had taken place in a plaster of Paris bandage, the patient, who formerly had not been able to touch the ground with his foot, walked now without the assistance of a stick and with scarcely any perceptible limping. In adults, however, is the resistance which the bones give to the fracturing power mostly so great, that it will be impossible by any means to produce the wished-for fracture. Instead of performing Rhea Barton's operation with the laying bare of the neck of the femur, it will be more advisable to follow up methods more similar to Langenbeck's plan in his so-called subcutaneous osteotomy. Four cases of this kind have lately been performed successfully in England.

The practical part of excision of the hip-joint has been greatly improved and simplified by Langenbeck's longitudinal incision. The injury hereby has become much less severe. The former incision encircling the outside of the trochanter, and which had been recommended by Anthony Wight, although it is often wrongly described as Velpeau's incision, is of a much severer nature. I have operated, including the operation during the war, about twelve times according to Langenbeck's plan, and I decidedly prefer it to any other method.

The patient being laid on his side with gently flexed hip-joint, a longitudinal incision is made over the great trochanter in a similar way as in excision of the elbow-joint over the olecranon. The incision touches the axis of the femur, and if elongated, would reach the posterior superior spine of the iliac bone. Its length varies from 2½ to 4 inches. Two-thirds of the incision are made into the glutaei muscles above the trochanter, and one-third on the trochanter. The glutaei muscles above the trochanter are then to be separated down to the neck of the femur in the direction of the longitudinal incision, until the neck of the femur and the margin of the acetabulum are entirely freed; the capsule is, in the first instance, merely incised in a longitudinal direction, and afterwards at both sides, at the margins of the acetabulum, slightly notched by a small incision; after this, by gently rotating the femur, the muscles inserting themselves at the trochanter are separated at both sides by the finger being inserted into the slit of the muscles and elevating the fibres of the same. Thus, above remains the fascia, and below, the capsule, in firm contact with the separated muscles. Lastly, the head has to be dislocated and sawn off, or if we do not succeed with the dislocation—any great violence must be avoided, for the atrophied femur is easily fractured in its middle—the separation has to be accomplished with a small saw (so-called "Stichsäge").

In Germany the sawing through is mostly accomplished in the neck of the femur, and the great trochanter afterwards removed if it be found diseased; in America and England, however, the femur is very often removed, on principle, below the great trochanter. Sayre, for instance, is of opinion the great trochanter ought always to be removed, as excision then would be followed by better result; the trochanter if left would prove like a cork to the wound, and would prevent the free discharge of secretions.

The statistics seem to verify this opinion. Out of 56 cases, where the operation was performed below the trochanter, 51·7 per cent. were cured, according to Leisrink's report, while out of 72 simple decapitations, nearly 33·3 per cent. recovered. Richard Good ("De la résection coxofémorale pour carie." Thèse de Paris, 1869) came to the same conclusion. Out of 49 excisions in the neck of the femur, 30 cases died, *i.e.* 61·23 per cent., and out of 56 excisions below the great trochanter, 27 only died, *i.e.* 48·21 per cent. If one should not feel inclined to attach any importance to these numbers, as yet being too small, though Otis likewise came to similar conclusions, it must be admitted, that in Germany the question whether it should be preferable to operate above or below the great trochanter has not been ventilated sufficiently in a scientific way. With regard to my own opinion, I must confess that the removal of the great trochanter, contrary to what one should expect, does not functionally give a worse result than the simple decapitation. Certainly, after the removal of the trochanter, a not inconsiderable formation of new osseous substance seems to be thrown out, or, under favourable circumstances, a head-like shaped bony excrescence is formed, which is not easily done after the mere removal of the head, as the trochanter in that case remains enveloped in a thick fibrinous mass. The best result which I ever obtained after excision of the hip-joint, and in which the patient made use of the newly formed joint almost as well as of a sound one, was in the case of a boy, where I not only removed the whole trochanter, but likewise gouged out the diaphysis of the bone, which had been affected by central caries to the extent of 1½ to 2 inches.

Roser's horizontal incision at the front part of the joint, an operation which, beginning quite close to the external side of the central nerve, divides the rectus muscle, the Sartorius, the tensor fasciæ latæ, etc., is not, according to my opinion, an appropriate one for the real excision of the joint, and principally as it merely gives you an opportunity, unless the incision is made very large, of performing the operation in the neck of the femur. The wound is situated hereby just in front, the discharge of the matter, therefore, must necessarily be less free. But as an incision for simply dilating the fistulæ, which are situated in the front of the joint, or for merely gouging out the joint by means of the sharp spoons, or for the extraction of the head that has been separated, I think this operation can well be recommended. I have performed it in such cases with success.

No excision, in fact scarcely any operation, exists which requires such great care and unwearied patience in the after treatment as excision of the hip-joint. The excised joint must be kept at rest, and the patient must be so laid that the wound at the hip is not pressed, and the surgeon can have free access to it, that the dressing and cleansing of the same can be accomplished without moving the patient. A plaster of Paris bandage, even if it be managed so that it envelopes the whole pelvis as well as the upper half of the sound femur, is not sufficient, according to my opinion. For as it is necessary to leave the whole part around the trochanter, where the large wound of the operation is situated, open, the femur very soon begins to slide upwards in the bandage, which after awhile gets loose, or impedes the exit of the discharge by getting just across the acetabulum, or even presses against it, and hereby produces pain, sets up irritation, and may even be the cause of the progress of the caries. During the first weeks it is necessary to keep the acetabulum and the surface of the femur well apart, and the soft parts well stretched in a similar way as in excision of the elbow-, the shoulder-, and the ankle-joints. The cleansing of the wound, as well as the formation of healthy granulations, make a more rapid progress, while afterwards pelvis and femur come into close contact in a similar way as in cases of excision of the ankle-joint, as soon as contraction of the granulations takes place, and are transformed into cicatricial tissue. In consequence of the above-mentioned sliding up of the femur, however, if this be not done the leg retains very often such a degree of adduction that the usefulness of the extremity may afterwards be severely jeopardised. I have been obliged in former years, while only applying the plaster of Paris bandage, to contend several times against these strongly marked symptoms of adduction of the limb, and have likewise seen them, and sometimes highly developed, in patients whose joints had been excised by other surgeons.

With children, according to my opinion, extension with application of weights and proper position of the limb, are for the present the best means to a satisfactory end. The little patient has to be placed upon a divided mattress, of which the two different parts, exactly corresponding to the spot where the excision was made, are separated by an interstice of several inches; on the side of the sound limb this space is again filled up to the middle of the patient's back by an extra pillow fitted to it. The interstice remaining on the side of the excision is covered by bed sheeting well pressed into the space, in order that the discharge can drop into it and thus be easily removed, and likewise the surgeon may be enabled to cleanse the wound without moving the patient. At the same time the buttocks rest on an air-cushion, horseshoe-shaped, so that it does not touch the operated part. Into the wound, which has been drawn together at the femoral end by a suture or two, a drainage-tube is introduced.

Quiet children rest thus quite comfortably, but now and then must be lifted in order to rearrange the sheeting, etc., which can be done without any difficulty. Adults and restless children must be supplied with a regular plaster of Paris bandage, as described above, with pelvic bandage, and enveloping the upper part of the sound limb, at the same time extension with weight should be applied, and, if necessary, the patient must be placed on a kind of rack described by me.* The sling of the adhesive plaster carrying the weight is placed underneath the bandage; the effect of the traction is not in the least interfered with by this firm bandage, as is proved by introducing the finger into the joint, for often in the same moment even, when the weight is removed, the finger is firmly jammed between femur and acetabulum.

I do not deny, however, that if a mattress or something could be invented by which the cleansing of the wound, the defæcation by the patient, and at the same time his utter immobility could be maintained, it would be a great boon. Esmarch has lately actually raised these patients by applying a plaster of Paris bandage to both legs, and which are then slung up, and only the trunk as far as the sacrum is supported by cushions. I scarcely believe that these means can be comfortable, and therefore can only be recommended for the worst cases.

The result after excision of the hip-joint in cases of caries which have well healed, is on the average satisfactory with regard to its functions. Billroth's description of the same is painted too black, according to my opinion. Certainly half of the cases are afterwards able to walk without the assistance of a stick, and almost the second half will be enabled to walk with the assistance of one; very seldom crutches are required for any length of time. It is true, however, that the sole of the boot on the diseased side has mostly to be raised, sometimes even to a considerable extent. Even in those cases of great debility and dangling condition of the newly formed adhesion, as well as in those where the muscles of the femur remain in a state of more or less atrophy, much can be accomplished by means of properly worked splints, to which a ring or plate is attached for the buttocks to rest upon in a similar way as in artificial legs in cases of amputation. Patients who formerly were obliged to depend upon crutches are generally enabled to walk after a time without the aid of a stick. This kind of apparatus, merely consisting of two strong splints, which are fastened with a moveable joint to the sole of the boot, and to which, above, a pelvic girdle is attached, is always worn by my patients for some time after excision, and even those who have recovered from severe coxitis, have to wear it in the first instance, in order to remove the weight of the body from the hip-joint.

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* In Billroth's and Pitha's System of Surgery.

Anchylosis scarcely ever takes place after excision of the hip-joint, and decidedly most rarely if continued extension has been practised.

With regard to the functional results after excision of the hip-joint on account of traumatic effects (gunshot wounds), we scarcely know anything. According to all probability, the result will be worse than after caries.

A most important point remains to be touched upon, and the more so as it repeats itself with respect to all the other joints, viz. the question whether, and how far, excision practised in children may impede the future growth of the diaphysis. We have to consider whether, after excision, the femur of the excised side grows in the same ratio as the sound limb, or whether the patient has the sad prospect placed before him of having the sole of the boot more and more heightened the taller he grows.

The long bones, as it is well known, principally take their growth from their two ends, viz. from the cartilage which is interposed between the epiphysis and the shaft of the bone. Besides, a certain degree of interstitial growth takes place in the shaft, if I am not mistaken; but it is decidedly of minor importance. Kölliker and others deny it altogether. In excision of the hip-joint, the whole interstitial cartilage between head and neck of the femur is removed; and in case excision is performed under the trochanter, the interstitial cartilage of this part of the bone is likewise removed. According to this, one might presume that after excision the femur would cease growing at its upper part.

To this I must reply that the excision of the hip-joint with regard to this point offers rather favourable circumstances, as the femur attains its growth much less at its upper part than at the epiphysis of the knee-joint. The energy with regard to the growing process of the two intermediary cartilages is—with the sole exception of the tibia, which seems to show equal propensities for growing at the upper part as well as at the lower —at the long bones very different at the different long bones. Radius as well as ulna take their growth particularly from the lower interstitial cartilage the same as with the femur, while the humerus takes its growth from the upper intermediary cartilage. If, therefore, both interstitial cartilages are removed, we have to expect, after excision of the knee-joint in children, the greatest shortenings—in excisions of the elbow-joint, however, the least. If one interstitial cartilage is merely removed, as in cases of excision at the shoulder- or hip-joint, we must expect a greater shortening at the former than at the latter one, etc., etc.

Altogether, our practical experience seems to agree with our theoretical reasoning. It is a pity that only so very few cases are recorded in which children, who have undergone the operation of excision, have been under observation for years after healing has set in; and very seldom indeed up to the time when the growing process altogether has ceased. We hospital surgeons, to whom the cases often are sent from long distances, and when cured generally are lost sight of, are not well fitted for these continued observations. As far, however, as I can judge from a careful perusal of my cases, I may say with regard to excision of the hip-joint, that the cessation of growth in the limb is not very considerable, certainly not more than we observe in cases of spontaneous cure.

After this, very often the growing process is likewise impeded in the diseased limb, the extremity does not grow in the same ratio as the sound one; and to the shortening of the leg, which may be produced by pelvic distortion, or by carious defects, or spontaneous dislocation, or something of the kind, may be added a more or less shortening of the bone itself.

From the foregoing remarks, one might conclude that the shortening merely depended upon the destruction of the upper interstitial cartilage of the epiphysis of the femur; and the more so as post-mortem examinations, as well as preparations in pathological museums, show that in severe cases of coxitis the carious defect very often extends much beyond this cartilage; that occasionally the head of the femur gets entirely separated from this cartilage during the suppurating and granulating processes, and that then not a trace of the cartilage remains; and that, lastly, in other cases again the inflammation may produce a synostosis between head and neck of the femur, after which the growth of the bone of course ceases. The careful examination of those patients in whom the limb has ceased growing, shows, however, that this supposition is not exactly the right one, and that with regard to the shortening of the limb the process at the interstitial cartilage of the femur is of minor importance. Without exception almost, the tibia of the affected side is likewise not of the same length as the sound one, and its shortening is sometimes equal to that of the femur—nay, I have seen several cases in which the shortening of the whole limb was principally due to the tibia. The foot remains likewise smaller than that of the sound limb. Unquestionably therefore we have to look for the principal reason of the impeded growth in the defective function of the diseased limb, viz. we have to consider it as an atrophy induced by inactivity.* The history of the different cases proves this fact.

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* I find that my opinion on this point is in opposition to that of my esteemed friend, Professor König, who, in his work on "Excision of the Knee-Joint in Children," does not believe in this kind of impediment with regard to growth. But he is decidedly wrong. The knee-joint was badly suited for deciding this question. A number of measurements undertaken on the shortened limbs of persons who have been suffering from coxitis of long standing will easily convince him that he is wrong. And certainly one dare not admit to him that on the supposition of impediments in growth in consequence of not using the limb merely, "unknown pathological, or pathological facts not easily to be accounted for, should be hidden from view." For the influence of functional irritation with regard to the nourishment of the different parts of the body is a general law, and nowhere is this influence more shown than in the muscular and osseous system.

The limbs which are principally affected in this way are to be found in those children who, on account of the severity of their malady, or on account of the sharp angle of the limb, have for a long time been prevented from using it.

Under these circumstances, the removal of the interstitial cartilage which may still remain is of small consequence at the excision of the hip-joint in children. If by excision the child should regain the use of the limb up till then useless, the operation may rather be the means of preventing than of favouring the impediments of growth. With regard to most of the other joints, circumstances are different. Hence the reason why I thought it desirable to discuss more minutely the importance of the interstitial cartilage with respect to the growth of the bone. In the following pages we shall be obliged to follow up the same question at each particular joint.

ON EXCISION OF THE KNEE-JOINT.

If we now come to consider the knee-joint, I undertake with still greater diffidence its excision than that of the hip-joint. It is true that the white-swelling of the knee is a disease of longer duration than coxitis, and that at the same time the treatment with plaster of Paris bandages, extension, etc., in some respects is of less use than in diseases of the hip-joint. The idea therefore is not far-fetched, whether it might not be advantageous to excise the joint in those cases which go on for a long time and yield to no treatment, and whether one might not even be justified in having recourse to this operation in those cases in which the joint has not reached the stage of suppuration, where the articular surfaces have not yet become carious, but where, nevertheless, the disease goes on steadily increasing, and where continually the joint is attacked by fresh inflammation.

According to my opinion now, the excision of the knee-joint is in those cases—at least with regard to us in Germany—an operation not yet to be justified. The mortality after this operation is with us much too large, while, after all, merely a small number of the chronic fungous inflammations of the knee-joint reach the suppurative stage with fistulous openings, manifest actual caries, and hereby really jeopardise the patient's life.

I am sorry to say we do not yet possess sufficient statistical records about the important point of either the frequency of the formation of fistulous openings, or the mortality in cases of fungous inflammation of the knee-joint.

In England—where in many instances different principles are followed up, and an excision of the knee-joint is sooner decided upon than with us, excision is even performed in the periarticular inflammation of the knee-joint—circumstances are seemingly different.

The opening of the joint certainly takes place more frequently, which I should feel inclined to ascribe to the circumstance that plaster of Paris or other firm bandages are less frequent, scarcely yet methodically employed; while, on the other hand, the result with regard to mortality is in England much better than with us. English surgeons save many more lives by excision than we do.

I do not think that this entirely and only depends upon the circumstance that English surgeons are in the habit of excising the joint at an earlier period; that the patients, therefore, are placed under better circumstances, are not debilitated so much by previous suppuration, and have not so frequently fallen into a state of tuberculosis. Although I will not deny that this circumstance may be of great importance—amputations likewise in England are followed by far better results than with us, and to compound fractures even fewer patients succumb —I feel inclined to think that the difference of race may have much to do with this particular fact. We notice, for instance, that the French bear wounds much worse than we Germans do, and the mortality after all severe operations and serious accidents seems to be much greater than with us. Another point however is, according to my opinion, the practical part of the after treatment in excision of the knee-joint, which, including perhaps all the capital operations, is decidedly better managed in England than in Germany; again in Germany better than in France.

How high the mortality after excision of the knee-joint in

Germany may be, cannot yet be ascertained exactly. I think we may not be far from wrong in supposing that up to the present time half of the number, or even more than half, have died in consequence of the operation itself; from the second half those cases even are to be deducted in which remaining fistulæ did not heal, and succumbed some years after from tuberculosis or albuminuria; and other cases in which, later, amputation had to be performed. Out of a number of 104 excisions of the knee-joint which have been exclusively performed by German surgeons, exactly 52—viz. 50 per cent.—died, according to the statistics of Mr. Knoll (Diss. inaug. Leipzig 1866). With children we fare better. According to the statistics of König, who has computed 122 cases of excisions in children (but performed by surgeons of all nations), 62½ per cent. were cured.

As matters now stand, it will be best to have recourse to excision in the knee-joint only in those cases where the disease begins to endanger life, where hectic fever has set in, the patient loses flesh, the existence of an intra-articular suppuration manifests itself, and a long-continued rational treatment has utterly failed. The number of fistulæ or abscesses in the immediate neighbourhood of the joint is of little importance with the knee-joint in deciding the question whether a conservative treatment may be of any avail, as at this joint mostly periarticular abscesses of the thickened tissue of the white-swelling may exist without grave implication of the joint itself. With the gouging out of the fistulæ and abscesses, followed by cauterisation, I have cured a not inconsiderable number of these unhealthy-looking, suppurating knee-joints, and not unfrequently with a tolerably moveable joint.

On the other hand, we meet with sad cases of atonic caries of the knee, with abundant and cheesy-like appearance of matter in the joint, and with early destruction of the ligaments, which are little inclined to discharge.

The prognosis after excision of the knee-joint however is, in my opinion, a very different one, according to the condition in which we find the opened joint. In a case of atonic caries, where the synovial membrane is scarcely injected, only slightly thickened, without any granulations, and where an abundant and even cheesy-like pus is contained in the joint, an operation is often followed by a putrid suppuration, with septic fever, where it is quite an exception for the patient to recover. It is very likely that by-and-by we shall amputate such a patient on the spot. If the joint, however, is filled with an abundance of red granulations—the inflammation therefore an entirely fungous one—the reaction which follows is much less severe. In such cases I have several times found that the fever which existed before the operation ceased entirely, or almost entirely, directly after.

In traumatic suppurations of the knee-joint, as they occur in civil practice, in consequence of penetrating, incised, or punctured wounds, or even severe contusions, I must condemn the secondary excision in the same way as I condemn the secondary amputation. The chances for the patient are neither improved by the one operation nor by the other, and I believe that a well managed conservative treatment, firm bandages with plaster of Paris, etc., ice, incisions made at the proper time, injections with concentrated solutions of nitrate of silver, and treatment with weights if necessary, will yet save most of the patients. I also do not feel inclined to abandon the conservative treatment if matters go wrong, when suppuration gets abundant, and burrowing abscesses are formed. If the patient is not able to stand a simple traumatic inflammation of the knee-joint, he will also not be able to bear the high amputation of the thigh which would now be necessary. Only quite exceptional circumstances would persuade me to deviate from this principle. I would amputate, for instance, if pyæmia set in. It is scarcely necessary to observe that the prospects for recovery are very slight indeed. Once, however, I was so fortunate as to save a patient's life by the high amputation of the thigh, who had had thirteen different rigors, a thoroughly icteric complexion, and was delirious. In civil practice I have only ventured once to perform excision in a case of traumatic inflammation. The case was quite an exceptional one. It happened to an engine-driver, who, jammed in by the engine which had been upset, had received so severe a burn of the knee-joint that, after removing the charred surface, not only the joint was laid bare, but even parts of the external condyle of the femur and of the patella, which had been destroyed by the burn, were exposed to view. This patient likewise recovered, and walks about well with his anchylosed and slightly shortened limb.

Excision of the knee-joint on account of gunshot wounds has been crowned in the late war with but little success. Most of the patients have died. Whether performed as primary or secondary operations, they have proved more dangerous than amputations of the thigh. Principally it has shown that in the majority of those cases in which excision might be permitted, viz. in small fractures, simple perforations, impressions and grazing of the condyles, comminuted fractures of the patella, and particularly in the pure injuries of the capsule—the conservative treatment gives us more prospect of success than the excision. Primary excision, from which, perhaps, one might expect most, is hereby already limited. It is a fact that during the last war a great number of perforating gunshot wounds of the knee, with or without injury to the bone, have recovered by conservative treatment. I have seen more than twenty cases myself. If excision, however, is only performed in those cases where conservative treatment has been tried and has failed, we are not likely to be pleased with the result. Excision of the knee-joint in military practice has therefore for the present been given up by most of the German surgeons. The greater number of those who had still performed their operation in certain cases as a final experiment during the Prussian-Austrian war, have abandoned it on principle in France. Such has been the case with me. In 1866 I excised the joint in three different cases, which now I should have treated, without exception, on conservative principles. All three patients succumbed—two of them some few days after the operation; the third, it is true, several months after a firm consolidation had taken place between tibia and femur, and I had been in hopes of saving this patient. I have to state, however, that we must not consider this question of excision of the knee-joint in military practice as a settled one; for the number of cases has not been large, and the operation has not been performed sufficiently often.

At last excision of the knee-joint, or instead of it the equivalent removal of a wedge-shaped piece of bone a little above the crevice of the joint, has been undertaken in cases of anchylosis with sharply curved angles, and when the brisement forcé could not be of any more avail. I do not perform this operation on account of its mortality still being too large a one, although some surgeons have been very successful with it—as, for instance, Ried—and because I have been successful until now in enabling patients, by different appliances, to walk without the assistance of crutches. In a case, however, of a strongly curved angle of the joint, where caries still exists, if it cannot be removed otherwise, and particularly when combined with dislocation of the tibia backwards, and strongly marked eversion of the foot, I might be induced to perform excision, where I otherwise should have followed the conservative treatment. I should much rather feel inclined to follow in osseous anchylosis a treatment which has lately been recommended from America, and been in that country successful in several cases, and is an imitation of Langenbeck's subcutaneous osteotomy. The operation is performed by making a hole straight through the obliterated joint by means of a drill; a strong steel is then inserted into the hole, and the joint is broken asunder.

For excising the knee-joint I recommend in all cases, and I prefer this method to all others, the slightly curved incision in front of the joint, which, beginning at the epicondylus of the one side, traverses over the tuberositas tibiæ to the other. After the flap has been dissected up, the knife dividing the ligamentum patellæ penetrates into the joint in order to cut the lateral and cruciated ligaments, the knee being strongly flexed, unless they have been destroyed by the suppuration. In following this method of incision one is enabled, without injuring in the least the soft parts, to remove with the saw, with the greatest ease, as much of the femur and tibia as altogether is permitted. No other joint, as is well known, gives us such limited space. One ought to apply the saw always quite in the broad surface of the bones. Should this not be possible—as, for instance, in gunshot wounds, where the splinters may reach into the diaphysis—we must instantly amputate the limb, which can quickly be accomplished by retaining the flap in front which was originally intended for the excision. A great advantage of this method is likewise the circumstance that both corners of the flap are situated as low as the anatomical circumstances permit. If the patella be sound, it should be retained and remain in the flap; if it be diseased, it should be afterwards removed. According to my experience, however, it is of great advantage if it can be retained, as otherwise the flap will become very thin, and adapts itself less well. I therefore have several times merely scraped the patella in cases of superficial caries, or removed a thin layer of it with the saw. It is important to make always the curved incision right down to the tuberositas tibiæ, in order that the flap, after adapting itself to the bone, may overbridge it well, and that the margin of the flap may not interfere with that of the severed bone. Healing by first intention, and preserving the same during the stage of suppuration—principally when the sharp margin of the divided femur is inclined to become slightly elevated—is hereby greatly favoured.

The different forms of the longitudinal incisions, all of which, however, I have practised on the living subject, are much inferior, according to my opinion, to the curved incision just mentioned. If, for instance, we have a case of white-swelling before us, with more or less thickened skin by infiltration, the longitudinal incision will have to be made of considerable length, and even then it will not be possible to get hold of the epiphysis without using much force, and thereby injuring the soft parts. The conditions are likewise not favourable for evacuating the discharge. The possible preservation of the ligamentum patellæ by employing the longitudinal incision can scarcely be of much importance, as the ultimate result of the operation will after all, with few exceptions, be the loss of the movements of the joint, viz. firm anchylosis. And, strange to say, in these cases—there may be, perhaps, four on record—in which, after excision, a moveable and yet strong joint capable of bearing the weight of the body has been the result, the ligamentum patellæ has oftener been destroyed than preserved. But there is another point to be considered. The result of excision of the knee-joint is at present in Germany so much less favourable than in England, that it does not well behove us to endeavour to improve on their greater or more favourable experience with regard to the practical part of the operation. The English surgeons always open the joint by broad incisions, which sever the ligamentum patellæ, and lay the joint well open in front, either by the U or even by the H incision. I think we do well if, for the present at least, we follow their example until our results are as good as theirs, and for the time being make no further experiments in Germany in the mode of operation.

In removing the epiphyses of the knee-joint, one should be as sparing as possible. It very often suffices to remove of the tibia merely a slice of the thickness of a thin pasteboard, or merely to gouge out the superficial layers by means of the sharp spoon, although, on the other hand, it will be necessary to take the remnants of the semilunar cartilages, in case they still be there, carefully away. Very sparing indeed we have to be with the removal of the bone in cases of children, and the more so the younger they are. The femur, as we have mentioned above, grows especially, and the tibia very energetically at the epiphyses of the knee-joint, and there is no part of the whole skeleton where the longitudinal growth of the body is going on to such an extent as near the knee-joint. The epiphysal cartilages are besides located so near to the free surface of the joint, that they have always to be sacrificed in extensive excisions of the joint. In consequence of excisions of the knee-joint in children, therefore, we decidedly have noticed the greatest impediments to growth.

Already in my article on the 'Diseases of the Organs of Locomotion,' in the "System of Surgery," by Billroth and Pitha, I have described and illustrated a case of Pemberton, in which by excision of the knee in a boy aged twelve, 2½ inches of the femur and 1 inch of the tibia were removed, and with it doubtlessly both epiphysal cartilages, and in which, six years after, the whole extremity was shortened by 9 inches. More minute details about similar cases we find in a work by Humphry and by König, who has lately again discussed this question.

In children, however, it is scarcely ever necessary to go with the excision beyond both interstitial cartilages. It is, moreover, an entirely erroneous idea to think it necessary to operate always in an absolutely healthy part, when we excise a large joint with a simple synovial sac. The fact that in caries of the shoulder- or hip-joint, the diseased glenoid cavity, or the acetabulum, is almost always left behind without any injurious effect, proves the contrary. In the subperiosteal excision, as practised nowadays, we leave on purpose the highly-diseased, fungous-like, degenerated synovial membrane behind, and even in those cases in which the disease originated in them, and the surfaces of the joint are only slightly affected. Excisions in chronic inflammation of the joint do not only act by removing every diseased part—as, for instance, in operations for cancer—

but a great deal of the effect is due to the operation itself. In the place of ulceration and fungous overgrowth, simple traumatic processes are substituted, which, according to our experience, are inclined to take a normal evolution, to end with regeneration and cicatrisation, viz. they are inclined to heal.

It is the question of a principle which is continually used in surgical practice—to give chronic, more or less specific, processes a different direction by inciting an acute traumatic inflammation. If it be different, it were not possible that lately, again, a distinguished English surgeon, Thomas Bryant, should have proposed to treat chronic fungous inflammations of the joint simply with extensive incisions, and in a great number of which he had the best results; and amongst them he mentions five cases in children between the ages of two and nine, who were afflicted with chronic fungous inflammation of the ankle-joint. The movement of the joint was preserved in all five cases. I should not feel inclined to imitate this proceeding in chronic cases, however beneficial it often may be in the acute stage. If we can get on in those cases without excision, it will be less dangerous, and certainly more effectual, if we have recourse to the gouging out of the joint through already existing fistulous openings, or small incisions made for that purpose.

Before I knew that excision might be combined with the most extensive removal of the spongy part of the bone, and I might risk leaving of the epiphysis a thin layer behind, merely consisting of the periosteum and a slight portion of the cortical bone, and this without any ill effect, it happened twice to me in excision on a child that I did not remove a very considerable part of carious bone, as I was not allowed to amputate, and the excision to a large extent would have frustrated every prospect of success; and yet both children, although slowly, recovered. Only with regard to the small joints of the carpal and tarsal bones it may be a serious matter, as may be easily understood, of leaving even small particles of diseased bone behind.

We therefore may lay it down as a rule, to remove in excision of the knee-joint in children merely at first a thin slice of bone, and, in case this should not suffice, not to go on with the saw, but with the gouge (spoon). The softened, broken-down osseous tissue has to be scraped out carefully, leaving the often much-thinned cortical substance with the periosteum behind, until we find at last the bone in a pretty healthy condition. The epiphysal cartilage is often by these means laid entirely bare from the side of the joint; we may even find it perforated by a fistulous opening, through which we have then to introduce a small spoon (gouge), in order to remove every diseased particle at a greater depth. Even extensive excavations of the whole broad part of the shaft are, in case of necessity, preferable to a very extensive excision, as the immediate growth of osseous substance seems to be more exuberant and the injury is well borne by the patient. In a case of very extensive excavation, it may be advisable to make a counter-opening at a proper part of the bone, or to put even a drainage-tube into the excavated bone. I have done this twice, and with success. In very young children it will often even not be necessary to remove any part of the tibia with the saw; we shall be enabled to take the diseased part away merely with the spoon (gouge). If the epiphysal cartilage can only partly be saved, it seems advisable not to sacrifice any more of the same than is actually necessary. Although we cannot be quite sure, yet experience seems to prove that even small remnants of the interstitial cartilage might be the means for the further growth of the bone. We have then to take great care that after the operation the two surfaces—or, in those cases where the cancellated substance has been removed by the spoon, at least the margins—of the bone are well adapted to each other.

Another question is, whether it is desirable that the two surfaces should be firmly pressed together in the bandage—whether they should be placed in direct contact; and in that respect, therefore, the knee-joint should be an exception to the rule which we follow up in cases of excision of the other joints. This question has not been sufficiently discussed—certainly not yet answered in a satisfactory way. One might well imagine, that by this the stagnation and putrefaction of the discharge in the narrow space between the two sawn surfaces might be favoured, and the rapid growth of healthy granulations from the surfaces at the same time might be prevented. I have therefore purposely left in the two last cases of excision of the knee-joint, and which were successful, a space the breadth of the small finger between the two surfaces. As soon as cicatrisation set fairly in, the two ends of the bones were nevertheless drawn firmly together. But of course we can form no positive conclusions from only two successful cases. One might well imagine that a greater separation of the sawn surfaces may easily happen in the after treatment of English surgeons, as they are not in the habit of applying a plaster of Paris bandage, and by means of their splints, etc., would scarcely be able to keep the bones in firm apposition. The application of a plaster of Paris bandage, as followed up by Langenbeck's school, in the after treatment, however, is without any doubt the most convenient one for the surgeon as well as for the patient. I am accustomed to apply it even before the application of the sutures, in the following way:—After thigh and leg have been well wrapped up in cotton-wool and flannel bandage, the front and sides of the joint merely being left uncovered at the points of incision, I apply a long and narrow plaster of Paris compress along the flexor side, which, commencing just below the tuber ischii, reaches almost down to the heel. Its breadth is so limited, that it does not touch the angle of the incision at either side. This compress is made by folding a long strip of lint twelve or fifteen times together, and dipping the same into plaster of Paris water, and is, as it adapts itself most minutely to the shape of the limb, decidedly preferable to any popliteal splint, even to that of Watson's. After this the compress is well fixed by applying plaster of Paris bandages round the whole limb. The front part of the knee is left uncovered. Before the bandage has got thoroughly hardened one must take care that thigh and leg are not kept in thorough extension, but are slightly bent, as this position will be more convenient for the future use of the stiffened limb. In order to give even a still stronger support to the front part of the bandage, I apply a large ⎍ shaped iron stirrup, which consists of a thick, one-inch wide band of iron, and which I myself always bend to suit each separate case. It overbridges the front and otherwise uncovered part of the joint, and is fixed to it at a distance of a hand's-width by plaster of Paris bandages. The foot has remained until now uncovered, and the flap has likewise been left unfastened. The flap is now turned back once more, the wound cleansed out, and the two surfaces of the bones well adapted according to wish. At last now, after making sure that all bleeding has ceased, the sutures are carefully applied, which certainly is a matter of some difficulty, on account of the large iron stirrup, but yet can be accomplished with perfect accuracy. The corners of the wounds are always left open; a drainage-tube is passed right across through the joint.

The bandage thus arranged, with an opening as free as possible in the neighbourhood of the joint, keeps the limb at perfect rest, and is of great firmness. If matters go on well, this bandage has to remain until perfect consolidation has taken place, without being changed during the whole time. In order to prevent the discharge from getting between the part of the bandage underneath the knee and the skin, two large pieces of oiled skin are fastened with collodion to the skin on each side of the joint beneath both corners of the wound, and as far as the plaster of Paris bandage is wanting, by which means the discharge is drained off into basins ready to receive it. The extremity is for this purpose either slung up by its foot-piece, or raised by a clump of wood or hard pillow in such a way that the space under the leg remains hollow.

In successful cases healing takes place almost without exception by forming a firm anchylosis between femur and tibia, but union is not always a bony, very often merely a strong fibrous one. Traces of passive mobility are not unfrequently to be noticed. If firm union does not take place we have to amputate the limb, or the extremity will be perfectly useless for support. Altogether failures after excision are nowhere so frequent as at the knee-joint. Suppuration goes on profusely, granulations which are growing out of the fistulous openings have an unhealthy aspect, the patient is feverish every night and gets weaker and weaker, in the joint no trace of consolidation shows itself, and at last we have to make up our mind to amputate. But even if everything goes on according to wish, the after treatment is a very tedious one. If occasionally a patient may get well in two months, generally three or four months are required until he can leave his bed on crutches, and five or six until he is enabled to walk with the assistance of a stick. In cases of long-standing caries the healing process is dragged on often to a great extent; sometimes it seems as if the condition of the patient had not been altered by excision. The reaction following is merely trifling, the joint attains quickly a certain degree of firmness, remains, however, bulky and swollen, and the fistulous openings as well as some parts of the incision discharge as freely as before. In a case of this kind healing was accomplished in a girl, aged seven, who was frequently afterwards treated with gouge and cautery, only at the end of the fourth year! Should healing have taken place the result is, with respect to the usefulness of the excised joint, on the average a very satisfactory one. This applies principally to adult cases, or, at least, excepting those of children where an impediment takes place in the growing of the excised joint, but which subject I need not dwell upon again. The patients are enabled to walk well with a thick sole to the boot, and generally better than with a partially anchylosed and partially dislocated knee-joint, very often the result of a carefully treated and spontaneously cured case. As a rule the extremity can bear a good deal of fatigue. The sole of the boot ought never to be made of such a thickness that it would equal the former length of the leg. If the knee is stiff the patients walk better with a shorter leg, because in that case they will not so easily, in walking, hit the ground with the tips of their toes. At last I must still mention that in all these cases, where ultimately no bony anchylosis has taken place, and if even only a trace of passive mobility is to be noticed between femur and tibia, it will be by far the best to let the patient wear for some time a firm apparatus for the knee. If this be not complied with, some unlucky dislocations and angular curvatures may by degrees take place, circumstances which it will be difficult, or even not possible to alter afterwards, and will impede the usefulness of the extremity. Principally in those cases, in which the healing of the limb was not accomplished in a straight, but in slightly flexed, and altogether, as we have seen, more favourable way, it is of great importance to have due regard to this point.

ON EXCISION OF THE ANKLE-JOINT.

In order now to conclude, I have now only to speak on excision of the ankle-joint. Of having this operation introduced into practice is only due to Professor von Langenbeck. For although there have been before his time, now and then, several cases operated upon, yet they were either merely uncertain experiments, not imitated by any other surgeons, or they were only extraction of splinters, or simple removal by the saw of pieces of bone not reducible after compound fractures and dislocations of the ankle-joint. Those cases of excisions undertaken by Astley Cooper, and which are frequently cited by other surgeons, have to be placed, according to my opinion, under this class. The typical excision of the ankle-joint is only known since that time, when Langenbeck, after the last Schleswig-Holstein war, put on record that wonderful success which he obtained through it, after the crushing of the bones constituting the ankle-joint. Since that time only has excision of the ankle-joint become common good of the surgeon, and one has commenced to propose certain indications for this operation, and to discuss its value against the mutilating operations and the conservative treatment. It is true that the discussions have almost only been going on in Germany; beyond Germany and even England, where the surgeons altogether are so fond of excisions, is the excision of the ankle-joint scarcely known up to this day.

Although our own experience of this operation dates only from not quite ten years, we are yet able to note one decided fact, viz. that the functional result of this operation altogether is a very favourable one, and certainly more favourable than with any other joint. The connection between the excised bones of the leg and the foot will become almost always so firm, that the extremity will be able to bear the full weight of the body, and equally well, whether a firm anchylosis or a moveable new joint be formed.

The number of cases at present on record is not large enough to judge with what frequency one or the other mode of healing takes place. Some surgeons are of opinion that anchylosis would be more frequent, and that opposite views were mostly based on fallacies, as after the loss of the mobility of the ankle-joint, the joint known by the name of Chopart's would gain a compensating mobility. All patients, whom I have operated upon, retained, however, all the desirable movements of the ankle-joint, and the articulating angle was, upon the average, very large. In one case, it is true, there was only an angle of 20 degrees, in several cases, however, three-quarters of the normal movement, or even more than that, was obtained.

Of great importance for achieving this favourable result may be the circumstance that no excision can be accomplished so easily and so well subperiosteally as the excision of the lower parts of tibia and fibula, where no muscles and tendons are inserted, and that on that account the regeneration of the removed bone is likely to become a more perfect one. This, particularly, is to be noticed with regard to the tibia, in which, altogether, we may say the regeneration of lost particles goes on with great activity. Even after extensive excisions, the formation of a very massive and broad epiphysis, with a thick malleolus, takes place ; the shape of the foot in the neighbourhood of the malleolus, therefore, if only some time has elapsed after the excision, and some part of the over-abundant callus has been absorbed, appears scarcely, or even not at all, altered. The regeneration of the fibula, viz. of the external malleolus, seemed to go on, in all cases almost which I saw, with a little less energy. This may partly depend, perhaps, upon the circumstance that after excision of the ankle-joint, very easily a slight degree of pes valgus is produced, which, however, does not interfere with the usefulness of the limb. It is questionable whether osseous regeneration ever takes place from the excised astragalus—we certainly ought not to expect anything of consequence.

Altogether the result of osseous regeneration after excision of the ankle-joint is so enormous, that, upon the average, three-quarters, or even more, can be reproduced of the part that has been lost by the operation, at least with regard to traumatic cases and secondary excisions. Not rarely is the shortening merely an eighth of an inch or thereabouts, or is even wanting altogether. I have myself seen a case in a young girl who at regular intervals, now and then, calls upon me. The frequently undertaken measurements do not show the least difference in the length of the two extremities. According to all appearance one can likewise remove large parts of the tibia and fibula without compromising the result of the operation and the future usefulness of the limb. The Austrian Captain Rochart received, on May 27th, 1866, at the battle of Trautenau, a gunshot wound, and the ball, smashing the tibia and fibula two inches above the malleoli, and producing a T fracture into the ankle-joint, remained in the joint. In this instance I excised, on August 14th, under seemingly very unfavourable circumstances—the heel besides showing a large sore—the ankle-joint by sawing a slice off the astragalus, and removing altogether about 3½ inches from the tibia and fibula, including the formerly extracted splinters. The limb shows after the healing, it is true, a shortening of 1½ inch. The ankle-joint, however, is normally shaped, is firm and actively moveable to a not inconsiderable degree, and the patient is enabled to be on his legs the whole day without any inconvenience. Similar extensive excisions have been performed by Langenbeck, Hüter, and other surgeons. A too moveable joint (Schlottergelenk) appears to be unheard of after excision of the ankle-joint. What altogether, however, an excised ankle-joint under favourable circumstances may be able to do and to bear, is shown in a most wonderful way in a girl suffering from hemiplegia.

M. S., at present aged fifteen, was attacked when twelve years of age with hemiplegia of the left side (facial paralysis, paralysis of the arm and leg). The paralysis began with anæsthesia of the little finger, and was complete after an elapse of ten or eleven days. Her intellect remained clear, the patient was not feverish, and her medical attendant thought at the time that hereditary syphilis was the cause of this attack. In the course of the next few months the facial paralysis improved very much, that of the leg in a slight degree, so that one can notice now some power of movement in the toes. The arm remained entirely paralysed, and the condition of the patient is to-day the same as it was then. About nine months after this attack, in the spring of 1869, a tense elastic swelling showed itself, according to the patient's opinion, in consequence of a distortion, on the right fibula just above the ankle-joint, and which had the appearance of a node. After having remained for a year in this condition, the swelling dispersed and was transformed into an ugly-looking, suppurating sore, at the bottom of which the deeply excavated carious bone was lying bare. In the beginning of May (1870), suddenly perforation of the ankle-joint took place, the joint secreted a fœtid discharge, and I then found the patient, when I was called in, in a most pitiable condition: the child was greatly emaciated and had high fever, the ankle-joint was dangling about, ligaments and cartilage were destroyed; the suppuration was abundant and of bad odour, and many fistulæ were burrowing along the inner side of the calf of the leg. On May 16th, 1870, I performed excision of the ankle-joint by removing about one inch from the tibia and one and a half inch from the fibula, and gouged out besides a good deal from the astragalus. In the beginning of September all wounds were closed, but only at Christmas the patient was allowed to walk, and soon then gained great proficiency. The movements are executed with great energy and accuracy, and the patient is able to walk about from morning till night without the assistance of a stick, and rests herself, on account of the paralysis of the other foot, almost entirely on the excised one. A shortening cannot be proved by measurement; the shape of the foot around the malleoli is so normal a one, that when covered with a fine stocking nobody will think that such a serious operation has been accomplished at this part.

Whoever witnesses for the first time an extensive excision of the ankle-joint, and perceives how, immediately after the operation, the foot is merely connected with the leg by a thin cylinder of soft parts, how every firm hold is wanting, how, in the real sense of the word, the foot is merely held by the skin, must necessarily feel the greatest admiration for the splendid aptitude of the human body by which it is empowered to repair such immense injuries.

But excision of the ankle-joint is not merely, with regard to its functions, a most wonderful operation, the mortality after it is likewise very low, the number of the mishaps a small one. Of about one hundred excisions of the ankle-joint which have been performed since Langenbeck's famous communications, during the last six or seven years, decidedly more than 75 per cent. of the patients have gained a useful limb.* I myself have performed the operation twenty-one times, and have only lost three patients by death; a fourth patient had to be amputated afterwards, and then recovered.*

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* With regard to the statistic of excision of the ankle-joint, *vide* the very carefully written dissertation of a pupil of mine, recording 104 cases (not

If we now consider the different ailments which take place in the ankle-joint, and which may induce us to have recourse to excision, the open wounds of the joint have decidedly to be considered first. These wounds and their consequences require most frequently excision, and produce likewise the most certain and best results. Compound fractures and dislocations of the ankle-joint, with a lacerated, comparatively large wound, and with protrusion of the ends of the bones, injuries as they occur in civil life, demand excision, if ever possible, at the first instance; for although I should not feel inclined to consider these injuries, as many surgeons do, equally dangerous as the penetrating wounds of the knee-joint, the mortality is nevertheless not a small one by treating them on the conservative principle. Some of these patients succumb early to septicæmia, some die from pyæmia, and a third class have to undergo secondary amputation, with or without success. By performing excision at an early stage the chances for preserving limb and life are, according to my opinion, greatly increased. During war primary excision will seldom be made, and I believe there are scarcely any primary excisions put on record—the patients are generally placed too late into our hands. There is no time to be found for operating, or even for making a proper diagnosis during the first day or two; and yet in those cases where the lower epiphysis of the tibia is severely crushed, primary excision would be the only means to reduce the mortality of these injuries, or to avoid secondary amputation. Of more frequent occurrence however are the penetrating and groove-shots of the ankle-joint, contusions of single parts of the joint-surface, and injuries of the astragalus—a great number of these cases get well under expectative treatment with little or, now and then,

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all lately performed however) of excisions: "On the Statistics of Excision of the Ankle-Joint," by Dr. L. Lauff, Diss. inaug. Halle, 1872.

* Beck mentions in his last capital work, "On Surgery of Gunshot Wounds," seven cases of excisions of the ankle-joint which were performed in the hospitals of Werder's corps. All seven cases got over the operation. One of these patients is, according to all probability, a sub-officer of the 13th Regiment, whose ankle-joint I excised with the assistance of Staff-Surgeon Krumm, in Dôle, and in whom the wounds nearly healed by first intention.

with no suppuration at all, or gives us at least full liberty, without risking much, to wait and see whether secondary excision will afterwards be necessary. It would therefore be very wrong to extend primary excision likewise to these cases, and the more so as the functional result after a secondary excision is decidedly a better one, as the reproduction of bone which takes place from the inflamed and thickened periosteum is generally more abundant, and the operation is borne very well indeed at that time by the patient. But although the results are so very favourable with regard to secondary excision, I am far from advising the same in gunshot wounds of the ankle-joint. The limits of conservative surgery may altogether be extended very far without injury to the patient as long as suppuration or fœtid discharge are not excessive, and the foot has not acquired such a position with the leg that the extremity is likely to become useless after the healing has been accomplished. This latter circumstance however happens less rarely with gunshot wounds of the ankle-joint than with compound fractures and dislocations in civil life which, originally produced by severe destruction and combined with extensive laceration of the ligaments, are brought to us after weeks, or even months, principally from the country, in such a neglected state that excision is the only chance for their recovery. The ankle-joint is perforated by numerous fistulæ, the foot partially dislocated, mostly placed in position of pes valgus; the internal malleolus, or a fragment of the tibia laid bare, has either become necrosed or is covered with a thin layer of granulations, and the external malleolus has been turned over in a more or less horizontal position.

Excision is for these cases the only remedy to produce a quick healing of the process, and to gain at the same time a useful limb, and no surgeon should hesitate to perform this operation as long as suppuration is still going on. I am, however, sorry to say, that in many of these instances, surgeons, instead of excising the joint, have recourse to the much more dangerous proceeding of amputating the lower part of the leg.

Another question is whether in injuries of that kind excision should even then be performed, when we get the patient under our treatment after healing has taken place, and all the fistulous openings are closed. I do not hesitate to answer in the affirmative, in case the deformity be of such magnitude that the foot cannot be made useful even by means of a mechanical boot. I, however, have had only once an opportunity of performing the operation under these circumstances. The case was that of Mr. K., aged 38, who exhibited a slanting fracture of the tibia penetrating into the joint, with crushing of the astragalus and calcaneus. In excising the joint at a later period, the greater part of the astragalus was found in the space between tibia and tendo Achilles in a crushed and flattened condition. After a very critical period, with profuse fœtid discharge from the joint and partial gangrene of its integument, healing was at last accomplished with bony anchylosis and great shortening of the extremity, but the foot was so much adducted (more than ninety degrees) that the point of the foot, as in the worst cases of congenital pes varus, was turned backwards. The bony union in the ankle-joint had to be separated during the excision with a chisel, and with the greater part of the astragalus, and the lower ends of the tibia and fibula, likewise a part of the formerly crushed calcaneus had to be removed. As, however, after the conclusion of the operation I attempted to give the point of the foot, which had been turned inward and backwards, a forward direction, this was found utterly impossible, although such large parts of bone had been removed. The skin had become, by the loss of substance, through gangrene, and by the scars, that had taken its place, so firmly attached to the bones of the leg, and the infiltration had likewise become so firm and unyielding, that a turning of some few degrees was scarcely possible. In order that excision should not be quite useless, I was obliged to detach tibia and fibula subcutaneously for about two inches more by means of a blunt raspatorium, and to make the skin moveable over them. After this had been done, it was possible to place the foot into a tolerably good position, and it was agreed to make efforts to procure a perfectly normal position by degrees, by changing during the treatment, at certain intervals, the plaster of Paris bandage. This was satisfactorily accomplished, and the patient was ultimately discharged with a very useful and well-shaped foot, and moveable joint, although it was shortened by one and three-quarter inch in consequence of the fracture and excision. The patient walks now daily one mile (German) with assistance of a stick.

Besides the traumatic cases, we have here to consider the spontaneously acute suppuration, and principally the osteomyelitic suppuration. Very frequently, nay, in the great majority of these cases, we shall be able to avoid excision; should, however, the discharge be abundant and fœtid, the ligaments be destroyed, etc., we shall do well to excise the joint as soon as possible. I have performed it under these circumstances twice successfully, and once with fatal result.

Much more uncertain are the results in cases of chronic caries. It must be well understood, of course, that only in those cases excision can be considered where the destructive process limits itself merely to the astragaloid joint and its neighbourhood, and the tarsal bones in front of the astragalus as well as its different joints are free from disease; this, however, I am sorry to say, is not at all the rule as soon as the disease has been going on for some time. In a similar way as at the hand, the chronic fungous and suppurative inflammations of the foot are greatly inclined to spread diffusely from one joint to the other. In examining the feet which have been removed by Syme's or Piragoff's operation, it is sometimes astonishing to what an enormous extent the destruction has been going on, and even in those cases in which the patients, if also with pain, have been seen limping about; and not very seldom do we find in these examinations in single tarsal bones, circumscribed processes of disease and suppurations of particular joints, which could not be diagnosed at the bedside. It is pretty certain that after excision of the ankle-joint and during the subsequent after treatment, diseased spots of this kind may take a retrograde movement and heal again, but it happens more frequently, that after excision suppuration and caries go on increasing and frustrate the success of the operation. Very extensive excisions, principally such as are combined with the removal of several tarsal bones, as they are undertaken in cases of wounds, and particularly after gunshot injuries, and successfully too, are scarcely admissible in cases of chronic caries. With regard to patients, already more advanced in life, the inclination for reproducing new bone is a very small one; on the surfaces of the section made by the saw, and which are mostly softened, unhealthy granulations spring up, the discharge is very abundant and of bad quality, the healing goes on very slowly, and soon are we convinced that Syme's or Piragoff's operation will produce a quicker and safer result.

It is quite different, however, with regard to younger children, in whom altogether such diffused suppurations as may happen in adults are of rare occurrence; the disease is much more limited to separate circumscribed spots. After the suppuration has been going on for years, I have often seen the worst cases recover even with partially moveable joints, without excision or amputation having been had recourse to, partly recovering spontaneously, and principally after the carious parts had been gouged out, as well as the different fistulæ, with the sharp spoon, and introducing subsequently drainage-tubes. The fistulæ are mostly so conveniently situated that one is enabled to enter easily the joint with the smaller kind of these instruments, and in case this should not be possible, two little incisions are merely needed in front of the malleoli in order to gain admission into the joint. The spoon often removes, besides a good deal of unhealthy-looking, œdematous, or yellowish, fatty, degenerated granulations, a not inconsiderable quantity of discoloured softened bone, and frequently it comes already in contact with entirely detached spongy sequestra; this happens more frequently here than in adult cases. The immediate result of the operation, therefore, does altogether not much differ from that of excision, with the difference, however, that the injury is much less severe; the reaction therefore passes over in a shorter time, the healing goes on at a quicker pace, and no alteration in the shape of the foot takes place, or any shortening of the same is produced. I am in a position to call the results, in fact, splendid, which I have achieved with children in cases of caries of the foot, and I have, therefore, never been under the necessity of excising the ankle-joint of younger children on account of chronic caries, just as I have likewise never performed partial amputations of the foot on account of this disease.

It will be plainly seen from what I have said that I am not greatly in favour of excision of the ankle-joint in cases of chronic caries. In children we gain our object, almost without exception, by adhering to conservative treatment, and with adults, particularly with those already more advanced in years, partial amputations of the foot (Syme, Piragoff) take us to our goal quicker and with greater safety. In case caries originate from an acute inflammation, is therefore not to be called a chronic one from the commencement, and happens to take place in persons that are still in the prime of life, we may, perhaps, more fairly expect a favourable result. In the pamphlet written by Dr. Lauff, thirty-three cases are mentioned in which excision of the ankle-joint was performed on account of caries; of these seventeen only were thoroughly cured (51·5 per cent.) and ten cases ended fatally. This result is decidedly much worse than what I have achieved in partial amputations of the foot.

Excision of the ankle-joint, if beforehand well practised on the dead body, is easily accomplished, and mostly done without great loss of blood, as tibia as well as fibula at their lower parts, where they have to be laid bare, are merely covered by skin. Of all methods, I can only recommend that proposed by Langenbeck, viz. the double longitudinal incision. It suffices for all cases, and does not injure the tendons in the least. One begins with the excision of the fibula. The foot has to be turned on its inner side and placed on a firm pillow, and the surgeon then makes an incision two or three inches long, just on the middle of the bone, down to the point of the malleolus. The knife has to be pressed firmly on to the bone in order that the periosteum should be penetrated at the same time. As soon as the knife has reached the point of the malleolus, the incision is carried for about a third of an inch still lower down, but by merely dividing the skin, in order not to injure the tendons behind the malleolus, but yet to be able to lift them off, and to have a free field for operating. After this the fibula is denuded at the spot where it is to be removed, and from there turn down as much as circumstances will permit of its periosteum by means of a raspatorium, and then the fibula itself is to be sawn through. The point of the index finger of the left hand, or a spatula placed into the interosseous space, have to protect the soft parts during the act of sawing. The saw itself is placed in a rather slanting direction on to the fibula, the point of it looking down towards the ankle-joint. By following this plan the malleolus when sawn through will be placed a little in front of the remaining part of the fibula, while in case of opposite direction it would be placed a little behind it; its later removal will be greatly facilitated by this proceeding. This is now done by taking hold of the upper part of the bone with Langenbeck's excision-forceps, or with a strong, sharp hook, and then separating it by moving it to and fro, partly with the raspatorium (blunt chisel), partly with the knife, from its lower connections. Herewith the more tedious part of the operation, and that part which requires by far the longest time, has been accomplished. The foot has now to be turned on its outer side, and the second incision has to be made on the inner side of the joint. This is done exactly in the same way as the first incision was done. It reaches down to the point of the malleolus, cutting through skin and periosteum, and then is carried still a little lower down, merely penetrating through the skin. The separation of the periosteum, merely slightly attached to the tibia, is more easily and rapidly here accomplished than from the fibula, and this is done only so far until the bone at the spot where it is to be sawn through is perfectly denuded at that part, and gives free access to the finger all round. The sawing is after this again done with the fine-bladed saw (Stichsäge), and the removal of the epiphysis accomplished by employing a strong forceps or hook. The firm ligamentous attachments at the point of the malleolus have to be separated by short perpendicular incisions made down upon the longitudinal axis of the tibia, and quite close to the same.

This method of sawing through the tibia *in loco* with the fine-bladed saw is decidedly to be done in all those cases in which, by chronic processes, the bones are thickened by deposits, or the integuments have become unyielding by inflammatory infiltration. In fresh traumatic cases, and likewise in acute suppurations of the joint, it may be possible, after the periosteum has been separated and the capsular ligament incised quite near to the margin of the foot, to have the foot gradually dislocated outwardly by an assistant, and that, by means of knife and raspatorium, the lower part of the tibia turns by degrees out of the inner wound, and can then be removed with the amputating saw.

Excision of the astragalus is only to be thought of in particular cases. In the chronic diseases, in which the cartilage is already entirely or almost entirely destroyed or separated, the thorough gouging of the joint's surface often suffices, which measure, in cases of severe rarefaction of the osseous tissue, can be easily accomplished by removing the whole prominent part which extends into the capsular cavity. In recent traumatic cases, however, it is preferable to remove this part exactly at the insertion of the capsule with the fine-bladed saw, which is introduced through the two incisions, which operation requires some dexterity on the part of the surgeon, as well as a firm fixing of the foot and good retraction of the wound by means of sharp hooks on the part of the assistants. In gunshot wounds of the astragalus, the different parts should be held by forceps, and removed by Cooper's scissors, or by the elevators.

I call those partial excisions of the ankle-joint, in which of the two ends of the bones of the leg, merely the one, either that of the tibia or of the fibula, is removed. This operation has to be considered almost exclusively in gunshot wounds, when merely one of the two ends of the bones has been crushed. About its advisability many controversies have been going on, but, according to my opinion, experience has already decided in its favour. Langenbeck particularly, but I myself also, have seen first-rate results following these operations. It seems, however, advisable to remove always in such cases the cartilage of the astragalus in order that the space between the joints be well separated, that neither retention of pus can take place, nor pressure of the suppurating bones may happen, and that at the same time a drainage-tube might be introduced straight through the joint.

After the operation has been finished and the hæmorrhage has ceased, the incised wounds have to be closed partly by applying a couple of sutures carefully above and below, and leaving merely the middle part open, through which, as already above mentioned, a strong single or double drainage-tube has to be introduced.

For the after treatment, a plaster of Paris bandage is the only useful one. I could dispense with it in the excision of any other joint better than in that of the ankle. For it is of great importance to fix the foot, which has lost its hold, through the operation, in the highest degree, in the exact way corresponding to the normal position—that is to say, the integuments have to be stretched in such a way over the bony loss, that the regeneration of bone may, in growing, fill up this empty space. The first bandage, if possible, has not to be removed before the fresh union has become firm. This can scarcely be accomplished unless by means of a well fenestrated plaster of Paris bandage. If this is done in the usual way, and merely two large pieces of the bandage are cut out corresponding to the spot of the malleoli, and therefore also corresponding to the two incisions of the wound, it will be difficult to keep the apparatus clean and sweet for a month or two. If, however, we likewise leave that part of the foot corresponding to the tendo Achilles, and which is desirable, uncovered by the bandage, as it is difficult to keep it clean from the discharge of the wound and the effect of the irrigator, it will be necessary to provide an extra support for the instep, if the bandage is to be of any use. A support of this kind is easily provided for in one way or the other by the surgeon who is familiar with the application of plaster of Paris bandages, and I therefore do not attach any particular importance to the plaster of Paris sling described by me some two years ago, although it has shown itself, during peace and war, in many instances of the greatest value. It merely shows one of the many ways by which our object can be attained, but has decidedly the advantage, that it may be easily managed by the surgeon who is less experienced in this kind of bandaging.

This appliance principally consists of two perfectly distinct parts, viz. a capsule made of plaster of Paris, which, encircling the leg, reaches upwards a little above the slightly flexed knee, and leaves off downwards just at the superior corners of the incisions, and a second capsule which surrounds the foot up to the commencement of the toes. The union of these two capsules is accomplished by means of an iron dorsal splint 1¼ inch wide, expressly made for each case by a smith, after a pattern of a long strip of lint prepared with plaster of Paris; this splint must correspond exactly with the shape of the leg and foot, and is fitted into the front piece of the capsule. The iron splint has fixed to its outer surface three iron rings, of which two, according to the necessity of each case, are used for suspending the foot in the hardened bandage by means of a cord, by which appliance the patient is enabled to gain some mobility in his bed without injuring himself in any way. The discharge of the wound runs into a basin placed underneath. In fixing the bandage the iron splint is placed direct on the leg, which has been protected before with a layer of cotton-wool, while the foot, well extended, is held in its proper position. The splint cannot produce any pressure upon the skin, as the limb, as soon as it is hung up, in consequence of its weight recedes to a small extent from the splint. After this the encircling is done with plaster of Paris bandages, as mentioned above. Should circumstances permit, a small part of the heel at least is to be included into this capsule; if, however, a bedsore exists at the heel, which often happens in neglected cases, particularly after suppurating fractures of the ankle-joint, or if fistulæ should exist at these parts, which likewise may be the case now and then, the heel has to be left entirely uncovered. In that case the front part of the capsule has to be applied still more carefully, and it will be expedient to support the heel with a sling of sticking-plaster in order to prevent the foot from slipping out unexpectedly. If the surgeon does not feel disposed to suspend the foot, the after treatment will be greatly facilitated if the bandage is managed in such a manner that the heel is not resting on the bed, and it is possible to place a basin for catching the discharge underneath the suppurating wound. This may be accomplished, for instance, after having applied a bandage, in a similar way as just described, by attaching to the sole of the foot a ⊥-shaped footboard with plaster of Paris; this board is three inches longer than the sole, and has at its lower end a crosspiece of one foot in length, which rests on the mattress. The iron splint may be replaced in this case by a stout plaster of Paris compress, which is applied from the beginning of the toes to above the knee. A short, thick pillow has to be placed underneath the knee, in order that the entire weight of the limb should not rest exclusively on the heel.

As soon as the wounds have closed and the cicatrix, which has taken the place of the former joint, has become firm and painless, the first experiments at walking may be tried with the greatest care. It is always necessary, however, to support the foot by a boot with irons, and to retain the movement, where a moveable union has been gained, with the utmost care. It is the best plan to fix at the external side of the foot, as well as at the internal, a strong steel splint, hinged at the ankle-joint, reaching up to the knee, and in cases where the foot is particularly weak, even beyond, and which, joined in the usual way by a narrow half-ring of iron at the back of the limb, are buckled with broad leather straps.

Baths, application of electricity (Faradisation), gentle exercise, and passive movements help to give strength and greater pliability to the foot. In order to induce the patient to move the instep freely, thus raising the foot from the ground in cases where no bony union has taken place between the foot and leg, I have lately used with the greatest success a gastrocnemius made of india-rubber and fixed to the boot. This consists of nothing else but of a strong india-rubber ring, which, placed behind the calf of the leg, is hidden by the trousers and fixed by two straps with buckles. The upper strap is connected with the iron half-ring as mentioned above; to the lower strap an eye is sewn, which is fastened to a hook attached to the heel of the boot. By either shortening or lengthening the straps, which of course acts upon the india-rubber ring, the power of this artificial muscle can be graduated entirely according to wish. The india-rubber ring presses the point of the foot downwards as soon as the patient raises the foot from the ground, and is lifted up again by the weight of the body as soon as the foot is put on to the ground, and by this alternate action the movement of the ankle-joint is produced. As this very cheap and solid apparatus is likewise instrumental for fixing the heel during walking, and for preventing the gradual sliding forwards of the calcaneus—for there is a certain tendency to do so in an ankle healed with a moveable joint—I can recommend its use most strongly.

There is no necessity for speaking any further about the excellent functional results with regard to excision of the ankle-joint. There is only one point I should like to draw your attention to, viz. that in all cases which I have seen and kept under observation for some length of time, the good result once achieved was never lost, nay, that even power, usefulness, and mobility were still going on continually increasing for some time. In excisions of the upper extremity, as it is well known, the contrary often takes places. But it is true that in the foot the conditions for preserving results once achieved, and their gradual improvement, are greater than in other joints. In my future lecture on excision of the shoulder- and elbow-joints, I shall take the opportunity to refer again to this subject, which has lately assumed so important a place in operative surgery.

ON DISEASES OF THE PHARYNX.

BY

PROFESSOR RÜHLE.

IN BONN.

GENTLEMEN,—The great frequency of diseases of the pharynx which I have observed here on the Rhine induced me to choose this subject. Just as I consider it my duty in my clinical demonstrations to occupy myself as often as possible with the daily occurring phenomena of medical practice, so I now ask your permission to treat of something near at hand rather than of a rarity. It would be of advantage to physicians as well as patients if it were the custom in the clinical wards not to perform, by preference, great operations, or to direct attention especially to rare cases. Medical practice does not offer a choice, and just as the physician cannot, or ought not to say, "this case does not interest me, it is only a simple catarrh, a mere headache," so ought the teacher also not to say it. The clinical wards should represent medical practice, the clinical teacher should be the physician thinking aloud. It is easier to exhibit striking cases and to rivet attention with them, but it is certainly not right.

Diseases of the pharynx also are often very slight local affections, but it is not unimportant to pay attention to them, as they may often become the cause of other deep-seated affections. On the other hand, we cannot fail to observe that diseases localised in the pharynx rapidly become connected with the most serious general symptoms. Call to mind only, Gentlemen, what a violent accession of fever a simple catarrhal inflammation of the pharynx is capable of producing in children, and even in adults; or that diphtheria may be accompanied by fatal fever even when it remains localised in the pharynx and respiration is not interfered with. Even where such fever was of short duration and when the inflammatory process is over, this disease is often followed by phenomena the severity of which appears inexplicable.

Cases of paralysis of the velum palati and muscles of the pharynx may be attributed to the disturbed nutrition of the muscular fibres in the neighbourhood of the seat of inflammation. This cannot be asserted with the same confidence of paralysis of the glottis. The cases of consequent paralysis of various parts of the body, and the occasional perfectly unexpected cases of paralysis of the heart, remain for the present inexplicable. Quite recently I saw a boy ten years of age who had had diphtheria of the throat only, which had disappeared after the inhalation of lime water. The fever had lasted five days, the appetite and general health were restored, and the boy was about to leave his bed. On the same morning, his paleness and debility attracted attention. The pulse was strikingly small, the breathing became deep and loud, the extremities cold, all stimulants were inefficacious, and in a few hours the asphyxia was complete.

The importance of the pharynx as a seat of disease is further proved by the fact that in most of the acute febrile general affections, it is implicated in part constantly, as in scarlatina and small-pox; in part pretty frequently, as in typhus and erysipelas. It is well known that in strong emotions and in almost all cases of narcotic poisoning, a feeling of dryness and roughness is felt in the throat. This does not always depend, however, upon actual dryness of the respective mucous membrane, as I have convinced myself by means of injections of atropine.

It is, perhaps, the vicinity of numerous important nerve-branches which become excited or paralysed by the local affection of the pharynx which causes such effects.

Another element in the importance of diseases of the pharynx, is their significance as a means of diagnosis. Just as we first recognise the development of scarlatina in the pharynx, so does the inspection of it furnish us with important suggestions in scrofula, syphilis, and sometimes tuberculosis, or, if you prefer it, phthisis. These constitutional affections also are reflected at this point. The pale granular swelling of the velum and pharynx, with the usual accompaniment of hypertrophic cicatricially contracted tonsils, belongs to scrofula; from the character of ulcers or cicatrices in the throat we infer the existence of syphilis generally incurable. There is, in fact, a form of that disease which manifests itself exclusively by constantly recurring affections of the throat. Excessive mercurial treatments are not followed by a more permanent result than milder ones; treatment with iodine or iodide of mercury is still followed by relapses, and it is a question rather of a prompt than of a very energetic interference. The pharynx also becomes affected in the course of phthisis. I do not venture, indeed, to call the circumscribed redness of the anterior pillars of the fauces characteristic, any more than the well-known pink line on the edge of the gums of the incisor teeth, or the clubbed finger-nails. But here, also, we meet with forms of ulceration which commence as yellowish nodes surrounded by a red, swelled areola, which become confluent and form a creeping, very irregularly shaped ulceration. In one case I have seen this spread forwards to the lips and gums, and I have twice seen the tongue attacked by such ulcers. But these proceeded from thicker, node-shaped infiltrations. In the post-mortem examination of such cases, as in that of almost all cases of phthisis, there was a mixture of miliary tubercles and infiltrations, with ulcerative destruction in the lungs and other organs. In the case of such ulcers of the throat and gums, there will always be a doubt as to the existence of syphilis. We cannot deny the possibility in each individual case. The combination of both, perhaps also the production of phthisis by syphilis, whether by inheritance or in the same individual, may no doubt occur. That deposits in the lungs arise from syphilis, I feel convinced. I saw a case in Greifswald, in which, in a female patient who had hectic fever, there was extensive infiltration in the apex of the right lung, complete aphonia, great emaciation, and anæmia. In the throat there were radiating cicatrices, and the larynx presented unmistakeable syphilitic ulcers. I treated this patient with mercurial inunction. The ulcers healed, the infiltration shrank, nutrition improved, and I was enabled to make use of the woman for several years for laryngoscopic demonstrations, because her defective epiglottis presented a very characteristic appearance. Later on, I heard nothing more of her. Here, therefore, the suspected phthisis was shown to be syphilis. But diseases of the throat and larynx are not always so characteristic. There is a form of disease of the pharynx which is distinguished by the great dryness of the darkly hyperæmic, but very slightly swelled mucous membrane. Ulcers gradually form in it and spread more and more until they reach the epiglottis and slowly destroy it, then advance into the glottis itself, are not improved by either antiphlogistic or astringent treatment, and do not always stand in any evident causal relation either to phthisis or syphilis.

Once in my life I have seen a chronic case of glanders of the throat and gums. When the patient was first shown to me and I saw this peculiar form of node with cheesy deposits, which, starting from small points, increased in size and formed extensive ulcers with callous edges, which also presented themselves in both the nasal passages, with considerable swelling of the neighbouring lymphatic glands, I asked him whether he had had anything to do with glandered horses. He was a proprietor of carriages for hire, and some of his horses had had glanders; but he asserted that he had not come into contact with any of them. It was decidedly not a case either of syphilis or tuberculosis. On further inquiry, it was found that at that time a blind horse, the favourite of its master, to which he often gave sugar on his hand and to which he therefore came near enough to inoculate himself, was declared sound by the examining veterinary surgeons and allowed to live, but was recognised later on as glandered and destroyed. Local treatment and the water cure so far improved the man's condition for a time, that he was enabled to follow his occupation for about a year longer. Iodine as well as arsenic acted unfavourably only; creosote was evidently the best local depurative; good diet was not forgotten. Unfortunately, no post-mortem of the body was made, and I am unable to inform you what later changes any of the internal organs may have undergone.

These hints may suffice, Gentlemen, to remind you of the importance of diseases of the pharynx for the recognition of anomalies of constitution.

Let us now turn our attention to the local affections of the throat and mouth. A large majority are catarrhal inflammations with more or less implication of the submucous tissue and of the tonsils. The sympathy of the latter effects the transition from a mere local affection to a general derangement of the system. The affections of the throat arising from purely local causes in otherwise healthy persons usually present no swelling of the tonsils. You are all fully aware what differences may present themselves in the degree of the redness, the swelling, and the secretion; how frequently the mucous membrane, its glandular apparatus, and the submucous tissue, finally undergo permanent changes which the frequently so obstinate forms of chronic catarrh of the throat present. The term "pharyngitis granulosa" so much used therefor is not happily chosen. Granular swellings occur in the most varied forms and have, of themselves, no pregnant significance. But those forms of catarrh in which we find in any part of the throat numerous, flat, pale-red elevations the size of a lentil are more obstinate and furnish a more copious discharge. These points of hypertrophy may have their starting-point in hypertrophy of glands. They often lie very concealed, *e.g.* in such a manner behind the pillars of the fauces that only a very full examination, with the tongue well depressed and the throat expanded during inspiration, enables us to discover them, or above and on the posterior wall of the velum towards the posterior orifice of the nose, where they can only be seen with the aid of a mirror.

I may mention here incidentally that, in acute inflammations with active sympathy of the submucous membrane, suppuration may occur in the latter and become very important from being deeply seated in the course of the posterior pillar of the fauces.

Putting aside the consideration of the diseases of the general system, the acute febrile (scarlatina, small-pox, diphtheria, erysipelas), as well as the chronic constitutional (scrofula, syphilis), we find that diseases of the throat are caused and kept up by various local lesions. I may remind you of burns, and of the use of strong solutions of tart. stib., which cause violent inflammations of the throat; and as regards the chronic forms, that they are much more frequent in males than females. And if I said on starting that I had been struck by the great frequency of catarrh of the throat here on the Rhine, so do I see the cause in the mode of life of the people. On the song-loving, *vine-clad* Rhine, cigar shops are by no means less numerous than in other neighbourhoods. Cigars, wine, and singing combined may well suffice to account for this frequency. There can also be no doubt that chronic catarrh of the throat is often met with in lecturers and preachers; the causes are here undoubtedly the same.

Next in order it is taking cold directly or indirectly which is dangerous for the parts in question. The neighbourhood of the throat may suffer directly, or other parts of the body may involve it indirectly. It appears to me to be the feet which most frequently play this part. The mucous membrane of the pharynx comes next after that of the nose as regards the frequency of such ætiological connection. The catarrh also very often extends, either from the mucous membrane of the nose to that of the throat, or *vice versâ*. After numerous returns, acute catarrhs finally become chronic, and in the course of these, acute exacerbations always recur. The mucous membrane of the throat offers a very common instance of the doctrine of the locus minoris resistentiæ.

It is by no means my intention to enter into the symptoms of individual diseases of the throat. The main point is recognisable on close inspection in a sufficiently strong light. We judge thereby concerning the degree, the seat, the acute and chronic form, and the special character of the disease, and often see the nature of the discharge. The more suppurative form of the latter often produces the bad breath, especially when the secretion lies in the deeper folds formed in the neighbourhood of the entrance of the glottis; an offensive smell from the nose may also result from a high-lying affection of the pharynx. That such secretion collects during the night, and especially in persons who have finished the day at a late hour with a complication of the above-mentioned deleterious things, cigars, wine or beer, and singing, is well-known to many, as well as that the efforts to expel it cause retching and vomiting, or that these are produced by the mere irritation of the firmly adherent copious secretion. Just as during the day its collection causes frequent attempts to swallow, so this may also occur in the night, and in many cases, perhaps, no inconsiderable quantity be passed into the stomach. A feeling of nausea is also occasioned in many cases by the swelling of the mucous membrane itself. I have seen cases of chronic catarrh of the throat in which a mere opening of the mouth wide caused retching. But that an elongation of the uvula, such as not unfrequently occurs in catarrh of the throat, can occasion obstinate nausea, vomiting, or even cough and hoarseness, appears to me very improbable. It is advisable, however, not to content ourselves with the condition of such a uvula alone in the cases we have to treat. The elongated uvula, like the congenital short frænum of the tongue and worms in the intestinal canal, is much more frequently an evil in the brain of the observer than in the body of the patient.

If in this manner considerable quantities of the secretions of the mucous membrane of the pharynx or posterior orifices of the nose, which by their retention often excite partly a reflex, partly a voluntary action of swallowing, pass into the stomach, we may fairly expect that the digestion may not unfrequently suffer therefrom. On improvement or removal of the copiously secreting affections of the throat, I have often seen a diminution of the loss of appetite, disinclination for food, and pain in the stomach after eating, even when the treatment has been a strictly local one, and recommend such treatment in all cases of dyspepsia accompanied by catarrh of the throat. The frequent recurrence of efforts to swallow causes, in other cases, much air to pass into the stomach in addition to the secretions. Anyone who has occupied himself with experiments concerning the act of vomiting after exposure of the stomach in dogs, knows how frequently the latter is greatly increased in volume by admission of air before vomiting occurs. The air enters by means of the movements of swallowing caused by the nausea which the copious secretions in the throat produce. In the same way, large quantities of air enter the stomach from too frequent acts of swallowing produced by other causes. We observe this best in hysterical patients, the rapidly increasing volume of whose stomachs has led many to imagine that it was occasioned by the secretion and absorption of gaseous products from the vessels of the stomach. The occasional great distention of the stomach may not only cause unpleasant and, in excitable persons, almost intolerable sensations, amounting even to difficulty of breathing, but may undoubtedly produce vomiting. I once became cognisant of this relation in a peculiar manner. I was consulted for chronic vomiting in a young lady, which obstinately resisted all the usual remedies and most careful diet. There was a moderate amount of catarrh of the throat present, and the patient complained of constant uncomfortable sensations in that region, but was not conscious of making very frequent attempts to swallow. Shortly afterwards she caught cold and had acute catarrh of the throat, accompanied by painful inflammation of the tonsils. The acute pain now prevented the attempts to swallow, and the nausea and vomiting ceased at once. This sufficed to explain to the patient, who was a sensible person, the cause of her obstinate stomach disturbance. The catarrh of the throat alone was treated, self-control overcame the evil habit of swallowing air, and the chronic vomiting ceased.

I followed a somewhat different course in the case of a very spoilt boy of fourteen. He not only kept his mother and sisters in dreadful anxiety day and night by his "terrible attacks," but also set various physicians to work. The attacks consisted in all sorts of convulsive movements, which were accompanied by cries and gestures of anxiety, and finally ended in vomiting. The boy really looked very pale, which was easily explained by the frequent vomiting which generally occurred soon after a meal. I convinced myself that the prelude to the attacks began with the swallowing of air. I succeeded at first in stopping these acts of swallowing by speaking seriously to him, and in thereby driving off the threatened attack, but the mother's, here negative, authority did not suffice therefor, and the boy thus escaped both school and work. I eventually regarded the boy as an adept in the art of causing vomiting, and as disposed to exhibit his "terrible attacks" caused as above described because he was pleased with the anxiety and care of those around him. I hope, Gentlemen, that this view of the case does not appear rough and barbarous to you, but I rather feel convinced that you have all had frequent opportunities of observing this irresistible impulse, which occurs especially in hysterical subjects, and naturally most frequently in the insane, in its various forms and degrees. I do not fear that I shall be thought to be talking nonsense if I assert that children may die of a bad bringing up, adults of —— well let us call it self-will. They "cannot make up their minds" to do what is required, to omit what is hurtful. Have you not frequently seen people who, from fear of pain, do foolish things, who from fear of discomfort (not merely from love, as in the "Elective Affinities") take less and less nourishment, and eventually fall into a state of inanition, which becomes fatal either directly or by the production of phthisis?

In the case just mentioned, I could not, of course, entertain the idea of treating the patient at home. I succeeded in having the boy sent to a cold-water establishment, conducted by an energetic colleague with whom I came to an understanding. By means of cold shower baths, and the fear of the rod, the attacks were stopped, by judicious training the impulse was conquered, and the youth is now the pride of his mother.

It now only remains for me to direct your attention by the aid of the above remarks to certain consequent phenomena produced by the swallowing of air and secretions into the stomach and often overlooked; to serious affections which may have their origin in a slight disease of the throat.

The so frequent cases of extension of the diseases of the throat to the tubes and to the organs of respiration are, it is true, brought about in a different manner. This is not the place to speak of the frequency of catarrh of the middle ear and the consequent impediments to vibration, upon which so many cases of deafness passing from one ear to the other depend. But it can hardly be denied that these are generally extensions of diseases of the pharynx or posterior orifices of the nose, and that a timely and efficient attention to catarrh of the throat in childhood would diminish the number of deaf people, I am quite convinced. At the same time, I do not believe in the usually asserted frequency of the connection by means of which hypertrophic tonsils are supposed to cause deafness by pressure upon the opening of the tubes.

The connection of diseases of the throat with affections of the organs of respiration is strikingly frequent. I have already mentioned that the disease may certainly also spread from the nose to the throat, and will only remind you here that the expired air which passes through the nose after coming into contact with the frequently ill-smelling secretions in the throat is perceived by the patients themselves, is painful to them, and gives them the idea that others may also perceive them and find them offensive, and that they will be shunned on that account.

Diseases of the throat may thus also lead to melancholy and avoidance of society. How much they produce hypochondria, I will mention later on.

There is only one disease of the throat which especially or quite exclusively runs its course into the œsophagus, *i.e.* the thrush. If the œsophagus becomes implicated in other cases, it is because the cause of the injury, *e.g.* corrosive fluids, is swallowed, and thus the œsophagus suffers as well as the mouth and throat.

Inflammatory processes and various forms of ulceration creep into the glottis, and not unfrequently further. The passage into the œsophagus must always first be opened by the act of swallowing, that into the glottis stands open, and only becomes shut in that act. So transient a contact as the swallowed secretions have with the mucous membrane of the œsophagus probably does not suffice, and on the other hand, the thicker epithelial covering and smaller supply of blood-vessels probably diminishes the vulnerability of the latter. On the other hand, particles of the secretions fly freely against the posterior wall of the pharynx to the arytenoid cartilages and posterior wall of the throat, extend to the false vocal cords and impregnate the glottis pressed down in every act of swallowing. The pillars of the fauces have further a direct connection with the folds, so-called ligaments of the mucous membrane of the opening of the glottis. Inflammatory swelling, œdema, and abscesses therefore meet with no resistance in this direction.

Allow me to remark cursorily that incomplete attempts to swallow frequently endanger and damage the respiratory passages; that a larger morsel than usual, a piece of meat, or potato, prevented either by its size or by an unsuccessful attempt to swallow from passing into the œsophagus, may remain lying upon the entrance to the glottis and cause rapid suffocation. Such sudden cases of death also naturally arise from the circumstance that regurgitating, or vomited substances coming from below sometimes overflow the entrance to the glottis in persons unconscious from chloroform or intoxication.

Acute inflammations of the pharynx with implication of the submucous tissue often cause very rapidly contractions of the entrance to the glottis, so-called œdema of the latter.

We once found a young man dead in his bed in the morning who had taken leave of his companions late at night apparently well, and certainly in high spirits. While suffering from catarrh of the throat, he had not refrained from going to a ball and drinking freely, and had remained too long in the cold night air, lightly clad. Under these deleterious conditions a rapid increase of the inflammation of the throat had taken place, extending to the ary-epiglottidean ligaments, the considerable swelling of which had caused suffocation. The alcoholic intoxication must have prevented the difficulty of breathing from bringing the patient to consciousness.

The respiration is certainly interfered with during sleep by diseases of the throat. Anyone who has observed children with hypertrophic tonsils reaching low down during sleep, will agree with me if I assume that respiration interfered with at that time by an unsuitable position, or in any other way, and frequently continuing, perhaps, for hours, is more damaging for the organs of respiration and the circulation of the blood, than an insufficient ventilation of the bedroom.

As regards the production of œdematous contraction of the glottis, moreover, it is much more frequent that we could know before the introduction of the use of the mirror. We very often see the beginning of this œdematous swelling in the mucous membrane of the arytenoid cartilages and ary-epiglottidean ligaments. Dyspnœa occurs only with the highest degrees of the swelling, the first conditions which may endanger life have often existed for some time. Not the slightest advantage of the laryngoscope is, that it enables us to recognise these first conditions, and to adopt an efficient prophylactic treatment.

The extension of the inflammation of the throat to the glottis in small-pox and scarlatina causes death in many cases, especially of the latter disease, and the same holds good for diphtheria and croup. If we succeed, therefore, in checking this extension, we may fairly claim credit for successful treatment.

Syphilitic affections of the throat may also reach the glottis through re-inoculation by means of the trickling downwards of the secretions. Morgagni already took this view, which is supported by the fact that the ravages of syphilis occur chiefly at the entrance to the glottis, but this is certainly not the only explanation.

In the course of chronic catarrh of the throat, we most frequently meet with the connection with disease of the glottis. The use of the mirror affords us the best information on this point. The trickling secretions alone may not only produce cough, but also changes in the voice which are characterised by their short duration, and present themselves especially on waking from long sleep. But if the mucous membrane of the glottis itself has been involved in the catarrhal affection, the phenomena will naturally be more constant, but from time to time more severe, however, from secretion occurring in the part itself.

The attacks of coughing occurring especially the first thing in the morning bring to light secretions of varying appearance, sometimes clotted and glassy-looking, sometimes muco-purulent and free from air, and these are only so far important as they contribute to the above-mentioned efforts to retch and vomit. The changes in the voice are sometimes considerable, the swelling of the false vocal cords sometimes suffices, even when moderate and with perfect integrity of the true vocal cords, to muffle the tones of the voice. The swelling of the posterior wall of the glottis may also, as Gerhard suggests, affect the voice, because on the approximation of the arytenoid cartilages a stiff fold is formed, and the degree of approximation necessary to produce true tones thereby hindered. In any case, it depends more, however, upon the vibration of the vocal cords themselves resulting from their proper degree of tension and sufficient prominence. We see in other cases that in spite of a sufficient approximation, a clear tone is not produced, the vocal cords being rather curved upwards by the expired air, and instead of being put into free tone-giving vibrations, present only flabby movements. It is a question here, therefore, of paralysis of muscles, and I am greatly inclined to think that such a condition enters largely into the production of the cases of catarrh of the glottis, acccmpanied by aphonia. When a moderate chronic catarrh exists, such as so often follows upon catarrh of the throat, it becomes at once necessary to make a greater effort for the production of the necessary tension of the vocal cords to overcome the existing obstacles. This increased effort, and the slight fatigue of the sympathising muscles, soon produce exhaustion of the organs of speech. Speakers, and especially singers, readily suffer pain from this cause, and it is intelligible that the latter, while losing the power of endurance, first lose their high notes. Laryngoscopy often shows no diseased condition in such a trained glottis, and yet the voice is wanting in tone, height, and endurance. Here the muscles alone are in fault, and this is proved by the result of appropriate treatment. I have already spoken of the paralysis of the vocal cords which follows diphtheria. After ordinary croup also a weak muffled voice often continues for a long time, and if we do not observe this in the milder degrees of inflammation, or in catarrhs, in the usual employment of the voice, it still interferes greatly even here with the higher claims made upon the voice by speakers and singers.

Just as enduring changes of tissue difficult of removal, thickening, etc., of the mucous membrane of the throat take place, so does this also naturally occur from catarrh of the glottis spread from the pharynx, and this also may prove to be a very obstinate evil which, although in itself secondary, is to the patient himself the chief thing, and constitutes for him the only ailment. But the removal of this condition is not effected without a simultaneous or previous treatment of the catarrh of the throat.

To judge with certainty of the chronic catarrhs of the pharynx and glottis, often accompanied by acute exacerbations, to acquire the necessary facility in the recognition of them often affords in practice the most satisfactory results. It is well known, indeed, that patients affected in this way often believe themselves to be phthisical, go in a melancholy mood to consult a physician, complain of cough, hoarseness, hæmoptysis, pain in the chest and debility, and expect to hear sentence of death passed on them. They form a great contrast to true consumptive subjects, who look upon cough and copious hæmoptysis, and expectoration as a "metastasis of hæmorrhoids to the chest." I am in the habit of telling my hearers that from this behaviour and these remarks of the patients, especially in reference to hæmoptysis, they might take the subjective consumptive patient for an objective hypochondriac, or sufferer from hæmorrhoids, the subjective sufferer from hæmorrhoids for an objective consumptive patient. It appears, in fact, that with these functional disturbances, which may be termed hæmorrhoidal disease, there exists a disposition to catarrh of the throat. Distended veins, especially, and relaxed swelling of the intensely reddened mucous membrane are more frequently met with in such cases than in other forms of that disease. The frequent hawking, cough, or retching gives rise to expectoration streaked with blood, to the great alarm of the patient, who carefully examines his pocket-handkerchief. I have never observed more considerable quantities of blood to proceed from the throat, such as are seen in phthisis, even in its earliest period. Neither do I believe, as yet, in the so-called "bronchial hæmorrhages," which may be compared to bleeding from the nose, and are said to represent the actual commencement of a case of phthisis. We may fairly ask whence the hæmorrhage came in such a case. There is at all events no proof of the occurrence of such bronchial hæmorrhages. From the glottis also, even in cases of ulceration, no considerable hæmorrhage occurs. The assertion of Louis, that hæmoptysis not caused by wounds, heart disease, or a suddenly arrested physiological secretion, such as menstruation, indicates the existence of phthisis, still remains in full force.

The fear of consumption which patients affected with catarrh of the throat feel is not, however, the only evil consequence, as catarrh of the pharynx and glottis not unfrequently produces a peculiar form of hypochondriasis. This will at once become intelligible if we consider the nature of this frequently so troublesome affection. The first requisite for the production of hypochondriasis is a sensation which attracts attention, in consequence of which conjectures arise as to the cause and importance thereof. The tendency to melancholy imparts to these conjectures the character of serious and dangerous disease, and in proportion as the patient has a knowledge of dangerous, incurable diseases in the regions in which his sensation exists, or has acquired such knowledge by reading or by questioning others, so does the fear and, eventually, the conviction arise that he must himself be the subject of such incurable disease. The sensation naturally does not remain confined to one situation, but the imagination of the patient becomes so fertile that there is at last scarcely any organ of the body which is not the subject of these melancholy but irrefutable conjectures. Side by side with the more general forms of hypochondria dependent upon digestion stand especially, as we know by experience, the sexual, the syphilitic, and the pharyngeal. In the latter form the course of things probably is, that the disease of the mucous membrane and the existing secretions produce the sensation, that the impediment to swallowing and consequent efforts to do so, the hawking, cough, and production of yellow or glassy expectoration, furnish food for the conjectures which, with the widely-known term of "laryngeal phthisis," bring the patient to the melancholy, but for that very reason perfect conviction that he is, or at least will become, the subject of that disease. The fear of cancer is more rare. Sometimes it is the more acute, intercurrent fear of suffocation, which wakes the patients in their first sleep on their becoming conscious of the movements of the pharyngeal secretion. They regard this dread as actual oppression of the chest, and speak of nightly accessions of extreme difficulty of breathing, which compel them to jump out of bed.

Catarrh of the throat, the secretions of which may maintain a bad smell, or even a bad taste in the mouth, sometimes produces in the more advanced forms of alienation hallucinations concerning taste or smell, which lead, for instance, to attempts at self-poisoning or to accessions of demoniacal frenzy.

The patients observe the foul taste of the poisoned food and medicine, or they smell the sulphur-fumes of the infernal regions, etc., etc. Whether affections of the throat may really determine originally the direction and character of the delusion, my small experience on the point does not enable me to say. As regards nervous affections of the region of the throat, we meet with hyperæsthesia as well as anæsthesia, spasm as well as paralysis, and increased reflex excitability. In general, various forms of excitation are combined, as are also various forms of depression. The former accompany the hyperæmic forms of disease of the mucous membrane, but may also occur without them in general nervous excitability, especially where there is irritation of the medulla oblongata, as in hydrophobia. The latter belong to the sequelæ of the inflammation or central affection. With them we sometimes find combined forms of atrophy, which the pale, thin mucous membrane enables us clearly to recognise as wasting of the muscles.

Intelligible as it is that hyperæmia and inflammation have an exciting influence over mobility and sensibility; that, as in *acute* affections of the larynx, spasm of the glottis and violent fits of coughing occur, and that pain, contraction, or increased reflex excitability also accompany such states of the throat, we still sometimes meet with these symptoms pretty strongly marked while the hyperæmia and inflammation are entirely wanting. In such cases, however, I have always found analogous increased excitability in other regions of the body, so that these neuroses appear to be only part of the phenomena resulting from more general functional disturbances of the nervous system.

The most familiar forms of paralysis are those which follow diphtheria. Nasal voice, imperfect efforts to swallow, difficulty of swallowing, a return through the nose of the fluids swallowed, are observed together with a pale, very relaxed velum palati, and the latter does not contract on being touched. But if retching occurs on tickling the throat, the palate and pharynx participate little or not at all in the movements. Progressive muscular atrophy also frequently exists in the cavity of the mouth. In two cases known to me, elderly persons, one of each sex, suffered from slowly progressing affections connected with paralysis of the parts in question, and in both the muscular apparatus of the tongue was implicated. The tongue lay in constant fibrillary vibrations flat and low in the cavity of the mouth, and could, in one of the cases, no longer be protruded as far as the front teeth; in the other case, the most prominent phenomenon was the difficulty of swallowing. The patient was able to swallow the first morsels of food or spoonful of soup, then the muscular power became temporarily exhausted, and she failed to swallow, and had to wait some time before the power of swallowing returned. In both cases there was muscular atrophy of other parts, of the hands, muscles of the back, and lower extremities, but it was most advanced in the muscular apparatus of the tongue and palate.

At this point, Gentlemen, I will mention to you, in conclusion, a case worthy of notice from its rarity. It occurred in a strong man of forty, who dated the commencement of his suffering six months back. At first he thought the secretion of saliva increased, then the act of swallowing gradually became difficult; he could no longer perform the movements required for smoking, no longer speak plainly, and as swallowing became more and more difficult, he suffered most from the constant secretion of saliva. This induced him to seek admission into the hospital here. There was paralysis, equally developed on both sides, but not complete, of the facial and glosso-pharyngeal muscles. The nervous current appeared more complete in the facial muscles, and the paralysis of the muscles of the throat was probably of earlier origin. There was not yet much atrophy of the paralysed parts. The velum hung down, and moved but little on being touched, or when retching was produced. The patient could swallow morsels of solid food with some effort, but could not succeed at all with fluids; so that all the saliva, the amount of which did not appear to be much increased, passed forwards through the mouth. The pronunciation of the consonants was so greatly interfered with that the patient could not make himself understood at all, although the tone of the voice did not appear weakened. It will easily be understood that the man was much depressed by this gradually increasing disease.

This case at once reminded me of that described by Wachsmuth, of diplegia facialis with progressive paralysis of the globe of the eye. There also all the phenomena had evidently been developed quite gradually; there was neither central disease of the brain nor any evidence of previous inflammations of the throat, syphilis, or paralysis of other parts. I despaired, therefore, from the first, of effecting any improvement even by treatment with electricity. This unfortunately proved true, as a six weeks' treatment with the continuous current had no effect. While in the earlier mentioned cases the paralysis plainly coincided with the atrophy of the muscles, and evident proofs of muscular disease were observed at other points, in this latter case the paralysis is very evident, but the atrophy not perceptibly developed; and this paralysis affects first the region of the glosso-pharyngeal muscles, then apparently extends gradually to the facials, probably also to the hypoglossals, and has its origin in the medulla oblongata in so peculiar a manner that only the above-mentioned nerve-centres, and not the branches given off by the brain for the spinal nerves, are affected. You will find detailed accounts of similar cases, described by other observers, in Wachsmuth's work.

From what I have said here, Gentlemen, you will have recognised, in the first place, my wish to remind you of the importance of diseases of the pharynx, which may depend partly upon the severity of the disease itself, but results especially from the concomitant and consequent phenomena which accompany apparently slight affections of the throat and give rise to conditions sometimes directly endangering life, sometimes proving of importance from their persistent consequences.

If, in conclusion, I make some remarks as to treatment, it is not for the purpose of recommending new remedies to you. I confine myself to the local treatment, and attach the most importance, in all modes of treatment, to the avoidance of everything which may do harm. For the generality of patients it may be difficult at first to give up smoking and drinking wine, but it is certainly beneficial. Local treatment with gargles, inhalations, painting with solutions, and cauterisations are then much more rapidly successful. To use gargles so that they shall reach all the diseased parts is seldom possible, and few persons know how to gargle so that the solution may really reach the back of the throat, although this is most certainly possible. The inhalation of vaporised solutions is, therefore, preferable. We may begin with solutions of common salt, then use mixtures of common salt with alum, and finally solutions of alum or tannin. The more prominent points must be touched with solutions of nitrate of silver, 1:20—40, or with the solid nitrate. The local treatment may be followed by a general treatment, or by such a one as it is customary now to carry out in some favourite watering-place, combined with the first. In very rare cases only do I consider general treatment alone sufficient.

For inveterate cases of throat disease, and especially for such as are purely local, I prefer sulphur-waters to those containing carbonate of soda, and see very satisfactory results from those of Weilbach, and, in slighter cases, from those of Ems and Neuenahr.

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